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Interpersonal Trauma Sequelae and Treatments:

The impact of emotion regulation and social connectedness on suicidal thinking, and effectiveness of group-based treatment.

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My boys, this is dedicated to you.

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Background

Interpersonal trauma is the direct harm to one person by another either through acts of commission (abuse) or omission (neglect). It often occurs in childhood, is usually repetitive in nature, and has significant negative impact on development. These symptoms are now recognised as distinct from Post-Traumatic Stress Disorder (PTSD) and have their own diagnostic category, however, there is insufficient evidence at present for clinical or policy recommendations to be made for the treatment of interpersonal trauma.

This thesis aimed to systematically review the literature on group-based treatment interventions for interpersonal trauma and their effectiveness in reducing trauma symptoms, expanding on previous research by broadening inclusion criteria beyond only Randomised Control Trials (RCTs). Furthermore, this thesis aimed to explore a theoretical model of the impact of childhood trauma on suicidal thinking, and how this association may be mediated or moderated by emotion regulation and social connectedness.

Methods

Thesis research aims were addressed in two studies. Research concerning group interventions for interpersonal trauma was systematically reviewed in Journal Article 1 through a PRISMA systematic search of electronic databases, with included studies rated for quality and their findings presented within a narrative synthesis. Journal Article 2 examined cross-sectional pre-treatment data from patients

receiving a group intervention for interpersonal trauma within an outpatient psychological therapies service using bivariate correlation, and mediation and moderation analyses to explore the proposed theoretical model.

Results

Twenty-four studies were included within the systematic review, with results highlighting that models of treatment were heterogeneous, but that large effect sizes were found for the reduction of trauma symptoms following group intervention. Journal Article 2 demonstrated that the impact of childhood emotional and physical abuse on suicidal thinking was mediated by emotion regulation skills and different types of group identification. The association between childhood sexual abuse and suicidal thinking was moderated by individuals' emotion regulation skills and their level of identification with their family.

Conclusions

Group-based interventions were found to effectively reduce trauma symptoms in both safety and stabilisation and reprocessing phases of trauma treatment, with further progression in treatment eliciting greater benefits. This type of intervention may be beneficial as a standalone treatment or as an adjunct to individual therapy. However, a clear need for further research with a greater degree of methodological rigour was identified. Findings suggest promoting adaptive emotion regulation and improving social connectedness can help to reduce suicide risk and the negative impact of childhood abuse, though these findings should be considered in the context of a specific trauma sample.

Research Portfolio Lay Summary

Interpersonal trauma, where someone is harmed by another person through abuse and/or neglect, often occurs in childhood and on a repetitive basis. This can have a significant effect on a person's development. They may find it difficult to manage their emotions, have lots of negative thoughts about themselves, and can have difficulties forming and maintaining relationships. Advisory bodies report that currently there is not enough high-quality evidence to support any one type of treatment for interpersonal trauma.

This thesis aimed to review the available research to investigate how interpersonal trauma is treated in a group setting, and how effective these group treatments are in reducing participants' symptoms. By examining pre-treatment data from participants of a group treatment for interpersonal trauma, this thesis aimed to explore the links between childhood trauma and suicidal thinking, as well as investigating how skills in managing emotions and relationships with family, community, and friends can impact these links.

Results showed that groups can be an effective way to reduce trauma symptoms, either as a standalone treatment or alongside individual therapy. Findings showed that being more able to manage emotions reduced the impact of childhood abuse on levels of suicidal thinking, and that identifying more strongly with family and friends can reduce how much some types of abuse lead to suicidal thinking. Further high-quality research is required, though this should not be limited to one specific type of study or group of participants. Overall, the results show group interventions are a valuable treatment for interpersonal trauma, especially when these support development of skills in managing emotions. Furthermore, helping people develop

social connections could be an important way to help reduce their risk of suicidal thinking in relation to their experience of childhood abuse.

Journal Article 1: Systematic Review[a]

Title: Group therapies for adults who have experienced interpersonal trauma: A systematic review

Short Title: *Group Therapy for Interpersonal Trauma*

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Highlights

- More research is needed to assess treatments of interpersonal trauma
- This review included quasi-experimental and observational studies, and RCTs
- Results showed effective treatment in both Phase 1 and 2 of a phase-based approach
- Continuing research with greater methodological rigour is required

Abstract

Background: The evidence-base for treatment of interpersonal trauma is currently insufficient for policy recommendations to be made. Meta-analytic evidence suggested potential efficacy of group-based treatments, however there are a number of concerns regarding potentially missed evidence and a lack of external validity when only RCTs are included within a review. As such, this review expands on this literature through examining evidence from quasi-experimental and observational studies in addition to RCTs.

Methods: A PRISMA systematic review with an *a priori* registered protocol identified 24 studies that met inclusion criteria. These were rated for quality and findings included within a narrative synthesis.

Results: Models for group treatment of interpersonal trauma were heterogeneous, with a number of studies producing large effect sizes for the reduction of trauma symptoms following group intervention. Group interventions focused on Phases 1 and 2 of treatment.

Limitations: Methodological limitations of the studies included in the review mean conclusions are tentative, with specific issues around replicability and generalisability of findings. Males were significantly under-represented in the included studies.

Conclusions: The current review offers support for the benefit of group treatments for both Phase 1 and 2 of treatment for symptoms associated with interpersonal trauma, contrasting with previous reviews. These may be offered either as a standalone treatment or as an adjunct to individual therapy. Continuing research with a greater

degree of methodological rigour is required to ensure evidence informing practice guidelines is of high-quality.

Keywords: group therapy, interpersonal trauma, CPTSD, phase-based treatment

Introduction

Definition of Interpersonal Trauma

Interpersonal trauma was initially defined by Courtois and Ford (2009) as involving direct harm and/or neglect by caregivers that is repetitive in nature, often occurring in childhood, with significant negative impact on development. The effects of this are often referred to as Complex Posttraumatic Stress Disorder (CPTSD), now recognised as a distinct diagnostic category within the International Classification of Disease – 11th Revision (ICD-11, World Health Organisation; WHO, 2018). It is distinct from PTSD in its resultant impairment across many domains, impacting upon an individual's abilities to function as expected for their chronological age. Models of CPTSD include core trauma symptoms (re-experiencing, avoidance of reminders, and increased arousal and hyper-vigilance) with additional 'disturbances in self-organisation' (DSO; Shevlin et al., 2017). Symptoms of DSO are categorised in to three domains: affect dysregulation, negative self-concept, and interpersonal difficulties.

Considering the overlap in definitions of trauma and the terminology used, specificity is important. For the purposes of this review, 'interpersonal trauma' is defined as harm caused by one or more individuals to another individual. This may be through acts of abuse or neglect and may occur at any point in the lifespan. Unlike in Courtois and Ford's (2009) definition, these traumas may not be repetitive; for example, a single incident of rape would be considered as an interpersonal trauma within our review. Furthermore, history of interpersonal trauma need not have resulted in deficiencies required to meet ICD-11 (WHO, 2018) diagnostic criteria for CPTSD. While we recognise these are common outcomes of the experience of

interpersonal trauma, participants of studies examined for this review need not meet these diagnostic criteria to be included within the review.

Impairment of skills in these domains can become cumulative. If an individual has not received appropriate modelling of affect regulation in childhood and adolescence (when these skills develop), sub-optimal emotion regulation may subsequently impact on their ability to relate to others in adulthood and sustain meaningful relationships, in turn impacting upon self-concept (Cook et al., 2005; Modecki et al., 2017). As noted, interpersonal traumas that cause such impairments are often repetitive, with evidence of a dose-effect response with greater number and frequency causing greater impairment and chronicity (Follette et al., 1996; Herman, 1992; Messman-Moore & Bhuptani, 2017).

Individuals who have experienced interpersonal trauma may also engage in a number of maladaptive coping strategies to reduce their distress. There are significant correlations between interpersonal trauma and suicidal behaviour, including serious self-harm, suicidal ideation, and suicide attempts (Briere et al., 2016; Easton et al., 2013; Geoffroy et al., 2014), alongside substance abuse (Sikkema et al., 2004; Sikkema et al., 2007), and offending behaviour (Ardino, 2012; Ball et al., 2013; Renn, 2002). Furthermore, interpersonal trauma increases vulnerability to poor physical health and mental health across the lifespan (Bosch et al., 2015; Kaminar et al., 2018; Mueser et al., 2004), and even in an individual's children's lifespan (e.g. Brunst et al., 2017).

Treatment of Interpersonal Trauma

At present, advisory bodies report insufficient high-quality evidence exists to support the use of any one type of treatment for interpersonal trauma or CPTSD (e.g. NHS Education for Scotland; NES, 2015). However, given what evidence is available, a phase-based approach is recommended, as proposed by Herman (1992, 1997). Trauma symptoms are treated in three stages. The first phase focuses on achieving safety and stabilisation of symptoms, usually incorporating psychoeducation and skills training to develop adaptive coping strategies (Dorrepaal et al., 2012; Zlotnick et al., 1997). Phase 2 involves the reprocessing of traumatic memories, allowing the individual to reframe the trauma narrative and subsequently reduce the frequency and intensity of trauma symptoms (Cloitre et al., 2011). Reintegration is achieved in Phase 3, where the individual begins to return to previously enjoyed activities and relationships (Herman, 1992; Cloitre et al., 2006). The treatment path is not linear and individuals may move back and forward between phases, spend varying durations at each phase, or may not visit all phases of treatment (Herman, 1992).

While some evidence has queried the usefulness of a phase-based intervention, particularly the necessity of Phase 1 (e.g. de Jongh et al., 2016), a survey of trauma experts by Cloitre et al. (2011) demonstrated 84% of respondents endorsed a phase-based approach, with evidence supporting phase-based treatment for CPTSD (Cloitre et al., 2012; Courtois & Ford, 2016), and highlighting that for CPTSD the affect management training received in Phase 1 is particularly important (Dorrepaal et al., 2014; Reddemann & Piedfort-Marin, 2017).

Individual phase-based treatment using different psychological models has been found to be effective in reducing trauma symptoms across both single studies and meta-analyses (Blackman, 2018; Cloitre et al., 2012; Cloitre, 2016; Zaleski et al.,

2016). However, receiving group treatment alongside others with similar experiences may have additional benefits for patients by normalising trauma symptoms and the use of maladaptive coping strategies, and can create a sense of solidarity between survivors (Katz, 2016; Kelly & Pich, 2014; Wallis, 2002; Zlotnick et al., 1997). As demand for psychological intervention continues to increase (British Psychological Society; BPS, 2016; Care Quality Commission; CQC, 2017; Scottish Government, 2018), group treatment may provide a means to increase access to psychological therapies without diminishing the quality of care received.

Previous Reviews

Reviews and meta-analyses examining the efficacy of group treatments for interpersonal trauma are sparse and limited to focusing on specific therapeutic models (e.g. Barrera et al., 2013; Sloan et al., 2013). Evidence from previous reviews suggests Group Cognitive Behavioural Therapy (GCBT) can significantly reduce trauma symptoms, though findings are variable as to whether GCBT offers any greater benefit to outcomes than treatment as usual (Barrera et al., 2013; Schwartze et al., 2017; Sloan et al., 2013).

Mahoney et al. (2019) conducted a meta-analysis of group interventions for CPTSD when considering trauma symptoms, depression, psychological distress, substance misuse, and dissociation as treatment outcomes. Their findings were limited by the low number of high-quality RCTs, but their meta-analysis suggested tentative support for Trauma Memory Processing (TMP, i.e. Phase 2) interventions for treatment of trauma symptoms, with non-TMP interventions beneficial for reducing general distress.

While Mahoney et al. (2019) provide a comprehensive overview of RCTs examining efficacy of group treatment for interpersonal trauma, there are concerns regarding the restriction of reviews to include only RCT studies. Although RCTs are traditionally viewed as the 'gold standard' for clinical research, Schlosser et al. (2007) report that despite often scoring well for minimisation of bias, RCTs can score poorly in measures of external validity. Even with trial registration and CONSORT protocols designed to improve methodological rigour and quality (Schulz et al., 2010), many studies fail to reach these standards and yet their findings are often still used to inform clinical guidance (Fox, 2017).

Konnerup and Kongsted (2012) argue that within social sciences, limiting inclusion criteria solely to RCTs will likely result in a loss of valuable evidence concerning the efficacy of interventions. To meet methodological requirements RCTs are often conducted in somewhat artificial settings where participants must meet specific criteria and as such are representative only of a small portion of the overall population. This is particularly problematic when using this methodology to infer efficacy of treatment interventions where populations are not so controlled, as seen in general clinical practice. Furthermore, the resources required to conduct these types of evaluation often lie beyond the scope of services who have a responsibility to meet high levels of need both in terms of the number of patients presenting and the complexity of their presentations. Employing a No-Treatment or Waiting-List control in these circumstances can be both practically and ethically challenging (Konnerup & Kongsted, 2012).

Aims of Current Review

Considering the increasingly important role systematic reviews take in informing both clinical judgement and policy decisions to ensure clinical practice is evidence-based (Shea et al., 2017), it is important to use an approach where valuable evidence is not overlooked. This is particularly relevant in areas such as the study of interpersonal trauma where the literature is less well-established and clinical guidelines have not yet been set. Unlike previous meta-analyses, this review is not limited to one therapeutic model or study type, which may allow for inclusion of evidence missed in previous work. Specifically, it asks:

1. What group interventions are used in clinical practice for the treatment of interpersonal trauma?
2. How effective are these interventions in reducing trauma symptoms?
3. Are there methodological sources of bias in the literature?

Methods

Search Strategy

Following PRISMA best-practice (Moher et al., 2009), an *a priori* protocol was registered with the International Prospective Register of Systematic Review (PROSPERO; registration CRD42018107675, see Appendix 2). The protocol was updated to include the narrowing of inclusion criteria to focus on outpatient samples, thereby minimising confounds generated by support received in addition to the group-based intervention within inpatient or forensic settings.

The first author conducted the search with training from an experienced librarian. A systematic search for primary papers was performed in PsycINFO, CINAHL Plus, Medline, Cochrane Central Register of Controlled Trials (CENTRAL), and Psychological and Behavioural Sciences databases in June 2018, including all relevant available studies up until that date. Searches used the terms ("Post Traumatic Stress Disorder" OR "PTSD" OR "Complex Post Traumatic Stress Disorder" OR "CPTSD" OR "Interpersonal Trauma") AND ("Group Therapy" OR "Group Intervention" OR "Group Psychotherapy"). Where limits were available searches were restricted to Adult or 18+ years. There was no limit on date or language of publication, or restrictions in relation to sample size.

Reference lists of previous reviews and of included papers were screened for additional studies, and authors contacted in cases where full-texts could not be obtained or to seek further unpublished work. A Google Scholar search was performed with the first 100 results examined for studies not already identified.

To be included within the review, studies had to meet the following criteria:

1. Study was a primary paper; books, book chapters, book reviews, systematic reviews and meta-analyses were excluded.
2. Interpersonal trauma was experienced by participants, defined as deliberate harm committed either by commission or omission by one person to another. This may include childhood sexual, emotional, or physical abuse, childhood emotional or physical neglect, witnessing domestic violence in childhood, adult sexual, emotional, physical, or psychological abuse, domestic abuse, harassment, stalking, rape, or torture. Combat PTSD was not included as research has suggested greater impact of contextual factors on symptoms in combat-related trauma (e.g. Olson et al., 2018). However, samples including

veterans or military personnel may be included where the treatment is for interpersonal trauma, not combat-related.

3. Studies explored effectiveness of a group-based treatment for interpersonal/complex trauma, where a group consisted of three or more individuals who were not related (i.e. not family therapy), and which used a therapeutic model with a basis in a relevant psychological model.
4. Treatment was delivered on an outpatient basis.
5. Participants in therapy were 18 years or over. No upper age limit or gender restrictions were applied. Participants must not have been affected by an Intellectual Disability or brain injury as this may impact emotion regulation skills beyond what would be expected following interpersonal trauma.
6. Treatment effectiveness was assessed using at least one quantitative outcome measure examining trauma symptoms, completed at a minimum of pre- and post-treatment following Moore et al.'s (2015) research guidance.

Titles and abstracts were scrutinised, with full-texts assessed for eligibility for any articles appearing to meet inclusion criteria. The first and second author discussed any studies where there was ambiguity as to whether the inclusion criteria had been met to come to a joint decision.

Data Extraction

A data extraction tool was developed by the first author to extract relevant details from each study using Microsoft Excel. The following data were extracted: country of publication, participant demographics (e.g. age, gender, diagnostic status, medication), general study characteristics (e.g. inclusion/exclusion criteria, name and

category of group intervention, presence of control, sample size, allocation protocol, attrition rate), and specific characteristics of the group intervention (e.g. patients and facilitators per group, number and duration of sessions, facilitator training, use of manualised format). Furthermore, data regarding treatment efficacy were extracted including testing time-points, outcome measures used, effect sizes and key findings. As not all studies reported effect sizes, Hedges' *g* was calculated as a standardised effect size for all studies given the range in sample size (Field, 2017).

Based on Herman's (1992, 1997) phase-based model, interventions were categorised by phase(s) of treatment delivered. Phase 1 interventions would also include treatments that provided support for comorbid mental health difficulties, such as substance use (e.g. Hien et al., 2009) or other serious mental illness (Mueser et al., 2007). Phase 1 and 2 treatments provided the same safety and stabilisation but also included the reprocessing of trauma memories. This may include Cognitive Processing Therapy (CPT; Bass et al., 2013), Cognitive Behavioural Therapy (CBT; Echeburúa et al., 2014), or specifically-designed reprocessing models, such as Warrior Renew (Katz, 2016). Only the study by Allon (2015) provided solely Phase 2 treatment, using an Eye Movement and Desensitisation Reprocessing (EMDR) approach. To be considered as having some aspect of Phase 2 work, the intervention would need to focus on reprocessing at least some of the traumatic memories.

Studies were not excluded from the review based on the presence of one or more control groups. For studies that included a treatment group as all or part of the control condition(s) (e.g. Classen et al., 2011), the group treatment focusing on trauma symptoms was considered the 'experimental' group, with general or non-trauma focused groups categorised as being a control group.

Risk of Bias Assessment

To assess overall methodological quality, an adapted version of the Agency for Healthcare Research and Quality (AHRQ) assessment tool was used (Williams et al., 2010) where key methodological criteria are retained but adjusted for the context of the current review. This is in-keeping with procedure in previous systematic reviews (e.g. Dudley et al., 2016). Methods used to select the cohort and comparison group (if applicable), calculate sample size, assess intervention efficacy, manage limited or missing data, and to control for confounding variables, as well as the description of cohort and intervention, were all assessed as 'Yes', 'Partial', 'No', or 'Unclear' (guidance document provided in Appendix 3). Studies were awarded an overall mean score as a numerical indication of quality. This was calculated by awarding two points for every 'Yes' rating, one point for every 'Partial' rating, and zero points for 'No' or 'Unclear' ratings. As not all studies included a control group, the total was divided by the number of categories rated (either nine or ten), resulting in a maximum possible score of two.

An independent researcher (LJSG) was trained in the application of the assessment tool before blind-rating 33% (n=8) of included studies, after which the researcher and first author met to discuss the ratings and any disagreements. Cohen's Kappa determined a good rate of inter-rater reliability (K=0.87). Total agreement was reached for 91.2% (73/80) of items with 5.0% (4/80) differing by one point, and 3.8% (3/48) differing by two points. Disagreements were largely due to subjectivity in details of reporting, and 100% agreement rate was achieved following discussion between the raters.

Data Synthesis

A narrative synthesis was conducted through which patterns in data collected were systematically investigated (within and across studies), including risk of bias (Ryan, 2013). Findings were synthesised in relation to the study outcomes: therapeutic approach, and efficacy in relation to PTSD symptom reduction.

Results

Study Selection

The literature search generated a total of 860 references: 368 in PsycINFO, 229 in Medline, 48 in CINAHL, 78 in CENTRAL, and 137 in Psychology and Behavioural Sciences. An additional 8 references were identified from other sources. Any duplicates found in one or more databases were removed, with a total of 24 studies meeting pre-specified inclusion criteria (see Figure 1).

Study Characteristics

Studies were conducted between 1997 and 2017 in the USA (15 studies, 62.5%), the UK (2 studies, 8.3%), the Netherlands (1 study, 4.1%), Spain (2 studies, 8.3%), Australia (1 study, 4.1%), Turkey (1 study, 4.1%), and Democratic Republic of Congo (2 studies, 8.3%). Sample sizes ranged from 8 to 405 participants (M = 94, SD = 91). Only four studies (16.7%) included men within their sample.

Table 1 provides an overview of included studies describing the authors, the location of the study, the treatment and control groups (where applicable), mean age and gender of the cohort, primary and secondary diagnoses, details regarding

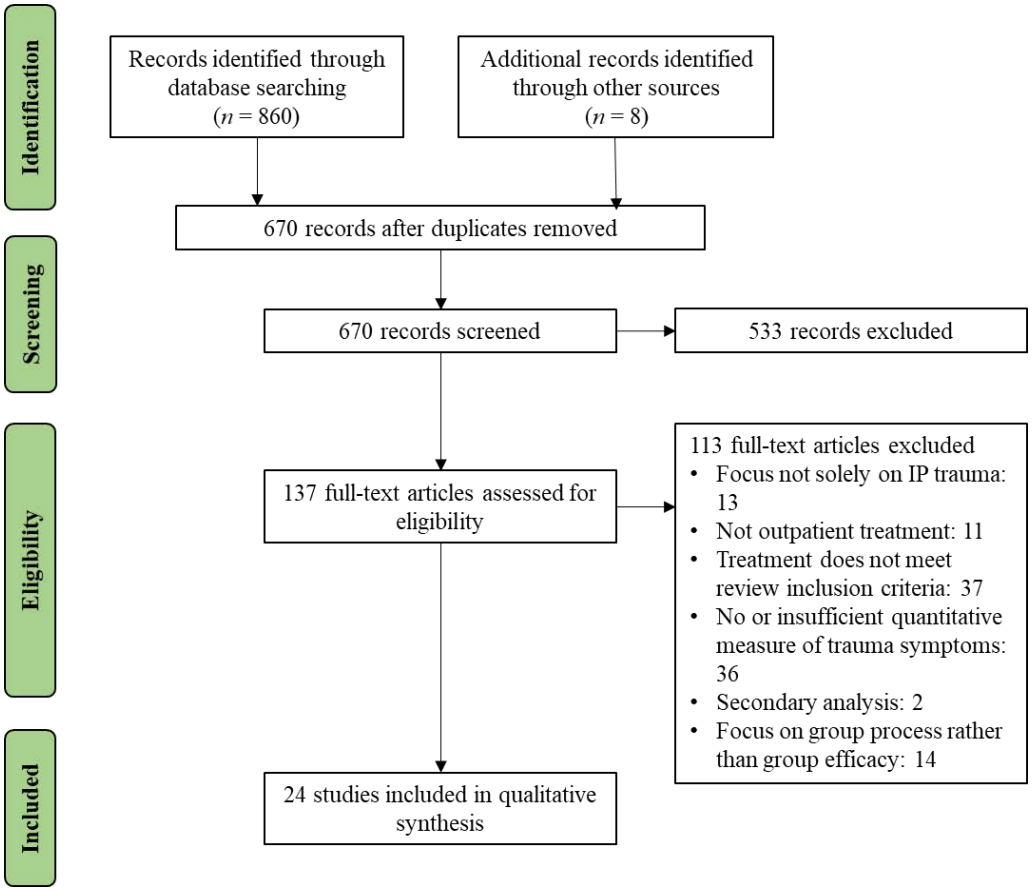

Figure 1. PRISMA flowchart for study selection.

implementation of treatment, outcome measures used, and timing of assessment(s). Studies are categorised by phase of trauma treatment (Herman, 1992). Table 2 shows the key findings and effect sizes for PTSD outcome measures for each study.

The majority of studies included at least some psychoeducational component. Thirteen focused on Phase 1 only, ten included some form of reprocessing thus providing Phase 1 and 2 treatment, and one study provided Phase 2 intervention only. None of the included studies involved components of Phase 3 work. Ten studies did not include a control condition (41.6%). Of those which included a control group, 71.4% used some form of treatment control (usually individualised therapy or support/skills group) rather than waiting list or delayed treatment only. Thirteen

studies (54.2%) provided formal diagnosis of PTSD, with the remainder characterising the sample as having a trauma history.

Phase 1 Treatments

All Phase 1 treatments aim to facilitate achieving safety and the stabilisation of symptoms. Four studies took a psychoeducation-only approach, providing participants with a greater understanding around trauma symptoms and the impact of trauma on day-to-day living (Ball et al., 2013; Brown et al., 2013; Dorrepaal et al., 2012; Lubin et al., 1998). Other than Brown et al.'s (2013) study, where insufficient data were provided to calculate an effect size, these interventions resulted in medium to large effect sizes for the reduction of trauma symptoms.

Sikkema et al. (2004) and Sikkema et al. (2007) employed the same approach but within a specific sample affected by HIV-AIDS. They described their results using domains of the Trauma Symptoms Inventory (TSI; Briere, 1995), which makes cross-study comparison more challenging, however in both studies reduction in symptoms across one or more domains of the TSI was found. Although there was insufficient data to calculate a standardised effect size in Sikkema et al.'s (2004) study, the Sikkema et al. (2007) study found reductions in intrusions and avoidance with small to medium effect sizes.

In addition to trauma psychoeducation, studies by Empson et al. (2017) and Hien et al. (2009) incorporated psychoeducation on issues around comorbid substance use through the Seeking Safety programme, with a reduced 12-session approach rather than the original 25 session protocol (Najavits, 2002). Both studies demonstrated reduction in trauma symptoms at the end of treatment, with medium to large effect

sizes. Though Empson et al.'s (2017) sample was small and specific – participants being transgender women with HIV – Hien et al.'s (2009) results were from a large, rigorous trial and using a broader sample. Therefore, the cumulative evidence of efficacy for reducing trauma symptoms can be considered fairly robust. However, Hien et al. (2009) also noted that Seeking Safety showed no greater benefit than a control group providing the same duration of treatment but focusing on women's health education, suggesting that for women with comorbid substance use at least, there are perhaps some factors of being in a group treatment that are valuable, separate from content-specific variables.

Two Phase 1 treatments supplemented trauma-related psychoeducation with greater focus on affect management and the development of emotion regulation skills (Wallis, 2002; Zlotnick et al., 1997). Both reported reduction in trauma symptoms following treatment. Effect sizes in Zlotnick et al.'s (1997) study were large, however Wallis (2002) did not report sufficient information to calculate effect sizes for these outcomes.

The remaining three studies included gender-specific content (Messina et al., 2012), or culturally-specific content (Kaslow et al., 2010; Kelly & Pich, 2014) in addition to trauma psychoeducation material. Messina et al. (2012) reported greater reductions of PTSD symptoms with a medium effect size in the gender-specific group, though both gender-specific and mixed-gender groups showed improvements in general wellbeing and self-efficacy. Culturally-specific interventions produced more variable results. Although some improvements in mood and trauma symptoms were reported, Kaslow et al. (2010) stated further modification was required for their treatment model to effectively treat trauma symptoms. Despite reporting positive appraisals

from participants, Kelly and Pich (2014) provided insufficient data to compute effect size for effectiveness in reducing trauma symptoms.

Phase 1 and 2 Treatments

Studies in this category used a wide range of therapeutic approaches. Most had a total number of sessions in the range of 16-27, though in some cases this was comprised of a mixture of group and individual sessions. Crespo and Arinero (2010) and Katz (2016) reported the fewest sessions, with 8 and 12 respectively. Most studies were explicit in describing exposure reprocessing strategies, via verbal or imaginal exposure, though some used more general group discussions to process trauma memories (Classen et al., 2011; Katz, 2016; Sayin et al., 2013), or exposure was done outside of group sessions, either at home or in individual sessions (Bass et al., 2013; Chard, 2005).

Bass et al. (2013) and Echeburúa et al. (2014) both used individual therapy as a control condition to their interventions. Bass et al. (2013) employed CPT with trauma memories explored using treatment principles at home rather than within the group, and Echeburúa et al. (2014) used Cognitive Behavioural Therapy (CBT). Both showed significant reductions in trauma symptoms post-treatment, with large effect sizes. Bass et al. (2013) noted group treatment outcomes were comparable to those achieved in individual support with therapeutic gains maintained at six-month follow-up. Echeburúa et al. (2014) offered individual treatment alongside the group intervention and so their conclusions are tempered accordingly; they suggest group treatment offered alongside individual CBT results in a greater reduction in trauma, depression,

and anxiety symptoms than individual treatment alone, both post-treatment and at 12-month follow-up.

Studies by Classen et al. (2011) and Crespo and Arinero (2010) compared group therapy to another group intervention used as a control; either treatment groups for non-trauma difficulties (Classen et al., 2011), or more general support groups (Crespo & Arinero, 2010). This allowed both studies to control for contact time and possible group effects within the experimental group. In both Classen et al.'s (2011) Trauma-Focused Group Therapy and Crespo and Arinero's (2010) cognitive-behavioural intervention, group treatment effectively reduced trauma symptoms with a large effect size evidenced in Crespo and Arinero's (2010) study. However, neither found significant differences in outcome between experimental and control groups.

Krupnick et al.'s (2008) Interpersonal Therapy (IPT) group approach and Chard's (2005) intervention of CPT specifically designed for survivors of sexual abuse (CPT-SA) both resulted in significant reductions of PTSD symptoms with large effect sizes, with this improvement maintained at 12-month follow-up in Chard's (2005) study. Both studies reported the group treatment produced results comparable to CBT interventions, with better outcomes than Waiting List (Krupnick et al., 2008) or Minimal-Contact (Chard, 2005) controls.

The remaining studies (Karatzias et al., 2016; Katz, 2016; Mueser et al., 2007; Sayin et al., 2013) did not include control conditions. Karatzias et al. (2016) and Mueser et al. (2007) both included individuals with a number of additional mental health difficulties within their sample. Mueser et al.'s (2007) CBT group and Karatzias et al.'s (2016) Trauma Recovery and Empowerment Model (TREM; Fallot & Harris, 2004), which draws on cognitive-behavioural principles, both reported effectiveness of the group in reducing trauma symptoms and some of the additional presenting problems,

Table 1.

Data extracted from included papers (*n*=24), grouped by phase of intervention.

Study (Year) *Location* *Study Type*	**Type of Group Treatment (*N*)** *Type of Control Condition (N), where applicable*	**Specific Trauma Experienced (% ppts. if applicable)**	**Mean Age at Baseline (SD)** *Gender (% Female)*	**Primary Diagnosis** *Secondary Diagnoses*	**Study intervention(s) with *N* sessions and session duration (mins)**	***N* Participants per Group** *N Facilitators per Group*	**PTSD Measure(s)**	**Additional Outcome Measure(s)**	**Timing of Assessment**
Phase 1 Interventions									
Ball et al. (2013) *Scotland* *Observational*	Survive & Thrive (*n*=24)	Sexual abuse (4.2%), Domestic abuse (12.5%), Multiple (54.2%), Familial substance misuse (4.2%), Unspecified (12.5%)	37.6 *100%*	Trauma History	Survive & Thrive: *n*=8, duration 90 mins	Max. 12 *2*	PCL-C	CORE-OM	Pre-, Post-Treatment
Brown et al. (2013) *USA* *Observational*	Psychoeducation (*n*=31)	Incest	37.6 *100%*	Trauma History	*n*=12 + weekly 1:1, duration NR	NR *NR*	TSI	MDI	Pre-, Post-Treatment
Dorrepaal et al. (2012) *Netherlands*	Psychoeducation (*n*=38) *1:1 Therapy (n=33)*	Childhood sexual abuse (97%), Childhood physical abuse (57%),	38.7 *100%*	PTSD *Depression, GAD, Social Phobia,*	Psychoed.: *n*=20, duration 120 mins *1:1: Variable*	8-12 *2*	DTS, SIDES	BPDSI-IV, DES	Pre-, Post-Treatment

RCT		Adult sexual abuse (58%), Adult physical abuse (46%)		*Panic, Substance Abuse*					
Empson et al. (2017) *USA* *Observational*	Seeking Safety (*n*=8)	NR	42.37 *100%*	Trauma History *Substance Abuse, HIV*	*n*=12, duration 120 mins	7 *2*	PCL-C	MAST-22, DAST-20	Pre-, Post-Treatment
Hien et al. (2009) *USA* *RCT*	Seeking Safety (*n*=176) *Psychoeducation around Health (n=177)*	NR	39.2 *100%*	PTSD *Drug and/or alcohol abuse*	Both: *n*=12, duration 75-90 mins	min. 3 *NR*	CAPS, PSS-SR	CIDI, SUI	Pre-, Post-Treatment, 1 w., 3 mo., 6 mo., 12 mo. Follow Up
Kaslow et al. (2010) *USA* *RCT*	Nia (*n*=130) *1:1 and support groups (n=87)*	Interpersonal violence	34.7 *100%*	Trauma History	*n*=10, duration 90 mins *1:1 + S.G.: variable*	3-5 *2*	DTS	BSS, BSI-GSI, ISA	Pre-, Post-Treatment, 6 mo., 12 mo. Follow Up
Kelly & Pich (2014) *USA* *Observational*	Psychoeducation (*n*=27)	Interpersonal violence (100%), Rape (72.7%), Assault with weapon (63.6%), Serious accident (59.1%), Witnessing severe injury or death (59.1%), Seeing dead bodies not in funeral	36.7 *100%*	Trauma History	*n*=6-10, duration 90 mins	NR *2*	PCL-C	ISA, CES-D, CDC BRFSS, GSE, IRPI	Pre-, Post-Treatment, 3 mo., 6 mo. Follow Up

		homes (54.5%)							
Lubin et al. (1998) *USA* *Observational*	Psychoeducation (*n*=38)	Sexual assault or rape (83%), Physical assault (59%), Violent accidents (31%)	41.5 *100%*	PTSD	*n*=16, duration 90 mins	6-7 *2*	CAPS	SCL-90-R, IES, DES, CMS, BDI	Pre-, Post-Treatment, 6 mo. Follow Up
Messina et al. (2012) *USA* *RCT*	Helping Women Recover, Beyond Trauma (HWR,BT; *n*=85) *Drug Court Treatment (DCT; n=65)*	Childhood sexual abuse (55%), Sexual abuse over lifetime (53%), Childhood physical abuse (37%), Domestic violence (66%), Torture (12%), Other non-interpersonal traumas (up to 70%)	35.9 *100%*	Trauma History *Drug/Alcohol Abuse*	HWR,BT: *n*=28, duration NR *DCT: variable*	NR *NR*	PDS	ASI	Pre-, Post-Treatment, Follow Up 18 mo. after treatment entry
Sikkema et al. (2004) *USA* *Observational*	Cognitive Therapy (*n*=28)	Childhood sexual abuse including unwanted touching or fondling, oral or penetrative abuse, sexual re-victimisation	44.6 *75%*	PTSD *HIV*	3-8 *2*	*n*=8 or 16, duration 90 mins	TSI	None	Pre-, Post-Treatment

Sikkema et al. (2007) *USA* *RCT*	Coping Group (CG; *n*=96) *Support Group (SG; n=101)* *Waiting List (n=56)*	Childhood sexual abuse, Adult sexual abuse, Rape	42.5 *54%*	PTSD *HIV*	NR *4*	CG: *n*=15, duration 90 mins *SG: n=15, duration 90 mins* *WL: N/A*	IES – Intrusion (In.) and Avoidance (Av.)	None	Pre-, Post-Treatment
Wallis (2002) *Australia* *Quasi-Experimental*	Specialty Clinics Group Therapy Programme (SCGTP; *n*=64) *Delayed Treatment (n=19)*	Childhood sexual, physical, or emotional abuse, Childhood emotional and physical neglect	39.3 *77%*	PTSD *Dissociative Identity Disorder, EUPD, Dissociative Disorder, Anxiety, Depression*	NR *2*	SCGTP: *n*=12, duration NR *DT: N/A*	TSI	None	Pre-, Post-Treatment
Zlotnick et al. (1997) *USA* *RCT*	Affect Management Group (AMG; *n*=16) *Waiting List (n=17)*	Childhood sexual abuse	39.0 *100%*	PTSD	6-8 *2*	AMG: *n*=15, duration 120 mins *WL: N/A*	DTS	SCL-90-R, DES, CTQ, SAQ	Pre-, Post-Treatment
Phase 1 and 2 Interventions									
Bass et al. (2013) *Democratic Rep. of Congo* *RCT*	Cognitive Processing Therapy (CPT; *n*=157) *1:1 Psychosocial Support (n=248)*	Sexual violence	35.4 *100%*	PTSD *Anxiety, Depression*	CPT: *n*=11 + 1 1:1, duration 120 mins *1:1: Variable*	6-8 *1*	PCL-C	HSCL-26	Pre-, Post-Treatment, 6 mo. Follow Up
Chard (2005) *USA*	CPT-Sexual Abuse (CPT-SA; *n*=36)	Childhood sexual abuse	32.8	PTSD	CPT-SA: *n*=17 + 10 1:1, duration 90 mins	NR	CAPS-SX, MPSS	DES-II, BDI-II	Pre-, Post-Treatment, 3 mo. + 12

RCT	*Minimal Contact (MC; n=35)*		*100%*		*MC: n=1, duration 5-10 mins*	*2*			mo. Follow Up
Classen et al. (2011) *USA* *RCT*	Trauma-Focused Group Therapy (TFGT; *n*=55) *Present-Focused Group Therapy (PFGT; n=56)* *Waiting List (n=55)*	Childhood sexual abuse including sexual coercion, attempted rape, rape, other unwanted sex	36.2 *100%*	Trauma History *Substance Abuse*	TFGT: *n*=24, duration 90 mins *PFGT: n=24, duration 90 mins* *WL: N/A*	NR *2*	PCL-S	SES, DAUI, SRBAS, IIP-32, TSI, PGI	Pre-, Post-Treatment, 6 mo. Follow Up
Crespo & Arinero (2010) *Spain* *RCT*	Exposure-Based (*n*=28) *Exposure Plus Communication Skills (n=25)*	Psychological (21.4%), Physical (3.6%), Psych. + Phys. (67.9%), Phys. + Sexual (0), Phys. + Psych. + Sexual (7.1%)	41.1 *100%*	Trauma Symptoms	Both: *n*=8, duration NR	NR *1*	EGS	BDI-II, BAI, STAXI-2, RSES	Pre-, Post-Treatment, 1 mo., 3 mo., 6 mo., + 12 mo. Follow Up
Echeburúa et al. (2014) *Spain* *Quasi-Experimental*	Cognitive Behavioural Therapy (CBT; *n*=57) *1:1 Therapy (n=59)*	Male intimate partner violence	41.77 *100%*	PTSD *Anxiety, Depression, Low Self-Esteem*	CBT: *n*=8 + 9 1:1, duration 60 mins *1:1: Variable*	4-6 *2*	EGS	STAI-S, BDI, RSES, MS	Pre-, Post-Treatment, 1 mo., 3 mo., 6 mo., 12 mo. Follow Up
Karatzias et al. (2016) *Scotland* *Observational*	Trauma Recovery and Empowerment Model (TREM; *n*=71)	Child sexual abuse, Child neglect, Physical abuse, assault, Domestic violence	40.4 *100%*	Trauma History	*n*=18, duration 90 mins	max. 10 *NR*	PCL-C	CORE-OM, RSES, DES, SCL-90	Pre-, Post-Treatment, 3 mo. Follow Up
Katz (2016) *USA*	Warrior Renew (*n*=32)	Childhood abuse (62.5%),	49.5	Trauma History	*n*=12, duration 120 mins	NR	PCL, PCL-5	PTCI, BSI	Pre-, Post-Treatment

Observational		Domestic violence (75%), Military sexual trauma (91%)	*100%*			*1-2*			
Krupnick et al. (2008) *USA* *RCT*	Interpersonal Therapy (IPT; *n*=32) *Waiting List (n=16)*	Sexual assault, Physical assault	32.0 *100%*	PTSD *Depression*	IPT: *n*=16, duration 120 mins *WL: N/A*	3-5 *2*	CAPS	HRSD, IIP	Pre-, Post-Treatment, 4 mo. Follow Up
Mueser et al. (2007) *USA* *Observational*	Trauma Recovery Group (*n*=80)	Childhood sexual abuse (63.6%), Adult sexual assault (63.1%), Attacked with weapon (62.1%), Attempted murder (74.2%), Situation where injured (50%), Situation where feared for life (53%), Close friend/family murdered (22.6%), Witnessed someone injured or killed (62.1%)	42.8 *79%*	PTSD	*n*=21, duration NR	6-8 *2*	PCL	PTCI, BDI, PTSD Knowledge Test	Pre-, Post-Treatment, 3 mo. Follow Up

Sayin et al. (2013) *Turkey* *Observational*	Eclectic (*n*=47)	Sexual abuse (100%), Childhood sexual trauma (40.4%), Rape (80.9%), Re-victimisation (40.4%)	31.7 *100%*	PTSD *Depression, EUPD, Panic, Eating Disorder, Phobia, OCD, Conduct Disorder, Vaginismus*	*n*=12, duration 90 mins	8-10 *3*	CAPS	DES, HAM-D, HAM-A, CTQ, GTFQ	Pre-, Mid-, Post-Treatment
Phase 2 Interventions									
Allon (2015) *Democratic Rep. of Congo* *Quasi-Experimental*	Eye Movement Desensitisation and Reprocessing (EMDR) Integrative Group Treatment (*n*=37) *1:1 Treatment (n=8)*	Sexual assault	NR *100%*	NR	EMDR: *n* = 2, duration NR *1:1: NR*	6-8 *NR*	IES	SUDS	Pre-, Post-Treatment

Note. NR=Not Reported; PCL-C=Posttraumatic Stress Disorder (PTSD) Checklist – Civilian; CORE-OM=Clinical Outcomes Routine Evaluation – Outcome Measure; TSI=Trauma Symptoms Inventory; MDI=Multiscale Dissociation Inventory; DTS=Davidson Trauma Scale; SIDES=Structured Interview for Disorders of Extreme Stress; BPDSI-IV=Borderline Personality Disorder Severity Index – DSM-IV version; DES=Dissociative Experiences Scale; MAST-22=Short Version Michigan Alcohol Screening Test; DAST-20=Drug Abuse Screening Test; CAPS=Clinician Administered PTSD Scale; PSS-SR=PTSD Symptom Scale – Self Report; CIDI=Composite International Diagnostic Interview for DSM-IV; SUI=Substance Use Inventory; BSS=Beck Scale for Suicidal Ideation; BSI-GSI=Global Severity Index of the Brief Symptom Inventory; ISA=Index of Spouse Abuse; CES-D=Center for Epidemiological Studies Depression Scale; CDC BRFSS=Centers for Disease Control Behavioral Risk Factor Surveillance System; GSE=General Self-Efficacy Scale; IRPI=Tilden Interpersonal Relationship Inventory; SCL-90-R=Symptom Checklist-90-Revised; IES=Impact of Events Scale; CMS=Civilian Mississippi Scale; BDI=Beck Depression Inventory; PDS=Posttraumatic Stress Diagnostic Scale; ASI=Addiction Severity Index-Lite; CTQ=Childhood Trauma Questionnaire; SAQ= HSCL-26= CAPS-SX=Clinician-Administered PTSD Scale – One Week Status Version; MPSS= DES-II=Dissociative Experiences Scale-2nd Edition; BDI-II=Beck Depression Inventory-2nd Edition; PCL-S=PTSD Checklist – Specific; SES=Sexual Experiences Survey; DAUI=Drug and Alcohol Use Interview; SRBAS=Sexual Risk Behavior Assessment Schedule; IIP-32=Inventory of Interpersonal Problems-32; PGI=Posttraumatic Growth Inventory; EGS=Escala de Gravedad de Síntomas del Trastomo de Estrés Postraumático [Severity of Posttraumatic Stress Disorder Symptoms Scale; BAI=Beck Anxiety Inventory; STAXI-2=Anger Expression Subscale from the State-Trait Anger Expression; RSES=Rosenberg Self-Esteem Scale; STAI-S=State Trait Anxiety Inventory; MS=Maladjustment Scale; SCL-90=Symptoms Checklist-90; PCL=PTSD Checklist;

PCL-5=PTSD Checklist 5 for DSM 5; PTCI=Posttraumatic Cognitions Inventory; BSI=Brief Symptom Inventory; HRSD=Hamilton Rating Scale for Depression; IIP=Inventory of Interpersonal Problems; HAM-D=Hamilton Depression Rating Scale; HAM-A; Hamilton Anxiety Rating Scale; GTFQ=Group Therapeutic Factors Questionnaire; SUDS=Subjective Units of Distress Scale.

Table 2.

Key findings and effect sizes from included papers (*n*=24), grouped by phase of intervention.

Study	**Key Finding**	**Underlying Therapeutic Modality**	**Reported Effect Size (PTSD Only)**	**Standardised Effect Size (Hedges' *g*)**
Phase 1 Interventions				
Ball et al. (2013)	Psychoeducational interventions delivered within holistic service provision may be useful in managing psychological distress in trauma survivors.	Psycho-education	*d* = 0.5	*g* = 0.54
Brown et al. (2013)	Group treatment may be beneficial as an adjunct to usual treatment for reducing PTSD symptoms and levels of dissociation.	Psycho-education	NR	Insufficient data
Dorrepaal et al. (2012)	Although treatment was effective in reducing trauma symptoms, there was minimal difference in outcomes for patients receiving group treatment compared to those receiving individual treatment.	Cognitive Behavioural Therapy	NR	DTS: *g* = 0.94, SIDES: *g* = 1.55
Empson et al. (2017)	A 12-session version of Seeking Safety can be useful in reducing PTSD symptoms and substance use in a sample of transgender women living with HIV.	Seeking Safety	NR	*g* = 0.59

Hien et al. (2009)	Seeking Safety was shown to be effective in reducing PTSD symptoms in a large and rigorous trial, with a sample of women with a history of trauma and co-occurring substance use.	Seeking Safety	NR	CAPS: g = 1.34, PSS-SR: g = 0.87
Kaslow et al. (2010)	Group therapy as an adjunct to individual treatment resulted in lower rates of depression and general distress, but requires modification to treat suicidal ideation and PTSD symptoms.	Psycho-education	d = 0.37	g = 0.38
Kelly & Pich (2014)	A flexible, non-manualised approach to group therapy for trauma may be feasible, though being a survivor was what bonded the group rather than ethnicity.	Psycho-education, Acceptance and Commitment Therapy	NR	Insufficient data
Lubin et al. (1998)	Group treatment was effective in reducing PTSD symptoms and psychiatric distress, with benefits mostly maintained six months later.	Psycho-education	NR	g = 0.82
Messina et al. (2012)	Gender-specific groups have positive effects on PTSD symptoms and perceptions of treatment, but both gender-specific and mixed-gender groups found benefits to general wellbeing and self-efficacy.	Psycho-education, Emotion Regulation Training	d = 0.4	g = 0.41
Sikkema et al. (2004)	More than 75% of participants showed improvement on one or more scales of the TSI (mostly in domains of trauma symptoms and behavioural difficulties) following group treatment.	Cognitive Therapy	NR	Insufficient data
Sikkema et al. (2007)	Participants in the experimental group showed reduced intrusions compared to the Waiting List control, and fewer avoidant symptoms when compared to the Support Group.	Cognitive Therapy	In.: d = 0.49, Av.: d = 0.23	In.: g = 0.45, Av.: g = 0.37
Wallis (2002)	Group treatment may be effective for the treatment of PTSD, and patient feedback was positive.	Psycho-education	NR	Insufficient data
Zlotnick et al. (1997)	Affect Management Training resulted in fewer PTSD symptoms and less frequent dissociation when combined with appropriate psychotropic medication and individual therapy.	Emotion Regulation Training	NR	g = 0.72

Phase 1 and 2 Interventions

Bass et al. (2013)	Group CPT was effective in reducing PTSD symptomatology and mixed anxiety/depression symptoms with benefits retained six months later.	Cognitive Processing Therapy	NR	*g* = 1.83
Chard (2005)	CPT-SA shows promise as an alternative treatment for survivors of CSA with significant reductions in symptoms of PTSD, depression, and dissociation immediately after treatment and at 12-month follow up.	Cognitive Processing Therapy	CAPS-SX: *d* = 1.52, MPSS: *d* = 1.55	CAPS-SX: *g* = 2.75, MPSS: *g* = 2.82
Classen et al. (2011)	The Trauma-Focused group resulted in lower PTSD symptoms than Waiting List control, but showed no difference in PTSD outcomes to the Present-Focused control group, though it did result in greater reductions of anger.	Cognitive Behavioural Therapy	NR	Insufficient data
Crespo & Arinero (2010)	PTSD, depression, and anxiety symptoms were reduced regardless of group condition.	Cognitive Behavioural Therapy	*d* = 1.47	*g* = 1.8
Echeburúa et al. (2014)	Individual CBT supplemented by group therapy resulted in better outcomes (reduced PTSD, depression, and anxiety symptoms) than individual treatment alone.	Cognitive Behavioural Therapy	NR	*g* = 1.09
Karatzias et al. (2016)	Group therapy may be useful for reducing general distress, PTSD symptoms, dissociation, and improving self-esteem, but it did not affect interpersonal difficulties, depression, or paranoia.	Cognitive Behavioural Therapy	*d* = 0.2	*g* = 0.22
Katz (2016)	There was high retention of group members and positive qualitative feedback, with some reduction in PTSD symptoms, though these should be interpreted with caution due to methodological limitations.	Cognitive-Experiential Therapy	PCL: *d* = 1.25, PCL-5: *d* = 1.49	PCL: g = 1.23, PCL-5: g = 1.43
Krupnick et al. (2008)	Group treatment outcomes (management of PTSD symptoms, improving interpersonal functioning) were significantly better than the Waiting List control, and are comparable to CBT individual interventions.	Interpersonal Therapy	NR	*g* = 1.27
Mueser et al. (2007)	CBT for PTSD in mixed-gender groups with patients who have serious mental illness is a feasible treatment, with significant	Cognitive Behavioural Therapy	NR	*g* = 1.05

	reduction in symptoms post-treatment and at three-month follow-up.			
Sayin et al. (2013)	Group psychotherapy is an effective treatment for women with different cultural backgrounds who have experienced CSA, with reduction in PTSD, anxiety, and depression symptoms.	CBT, IPT, Narrative Therapy, Psycho-education	NR	$g = 1.28$
Phase 2 Interventions				
Allon (2017)	Individual sessions of EMDR were more effective than two group sessions of EMDR-IGTP in reducing subjective distress.	EMDR	NR	Insufficient data

Note. NR=Not Reported; DTS=Davidson Trauma Scale; SIDES=Structured Interview for Disorders of Extreme Stress; CAPS=Clinician-Administered PTSD Scale; PSS-SR=PTSD Symptom Scale – Self-Report; In.=Impact of Events Scale, Intrusion subscale; Av.=Impact of Events Scale, Avoidance subscale; CAPS-SX= Clinician-Administered PTSD Scale-One Week Status Version; MPSS=Modified PTSD Symptom Scale; PCL=PTSD Checklist; PCL-5=PTSD Checklist 5 for DSM-5.

with a small effect in Karatzias et al.'s (2016) study and a large effect size in Mueser et al.'s (2007) study. Katz's (2016) intervention was specifically designed for women who had experienced military sexual trauma, which gained positive qualitative feedback and some reduction in trauma symptoms, however Katz (2016) notes these should be interpreted with caution due to the methodological limitations of the study. Sayin et al.'s (2013) group treatment was defined as eclectic psychotherapy drawing on "CBT, IPT, narrative therapy, psychoeducation, and expressive techniques" (p4). They reported the group intervention as effective in reducing trauma symptoms in women who had experienced Childhood Sexual Abuse (CSA) who are from different cultural backgrounds. The standardised effect size suggested a large effect of treatment on PTSD symptoms.

Phase 2 Treatment

Allon (2017) was the only study to examine the efficacy of Phase 2 work alone, using a group EMDR protocol to deliver two sessions of group treatment, with individual EMDR treatment used as the control condition. Number of sessions was limited due to external constraints, however across this short period the evidence suggested individual EMDR was more effective in reducing PTSD symptoms than group-based EMDR. There was insufficient data provided to calculate any effect size of these outcomes.

Treatment Modalities

A broad range of therapeutic modalities were employed across included studies (as shown in Table 2), with three studies (12.5%) explicitly acknowledging their

intervention drew on several different modalities (Kelly & Pich, 2014; Messina et al., 2012; Sayin et al., 2013), and others advising their interventions, which were tailored to a specific demographic, were informed by one model but with relevant modification (e.g. Kaslow et al, 2010; Katz, 2016). Others used disorder-specific therapies, either in consideration of comorbid substance use (Empson et al., 2017; Hien et al., 2009) or for trauma (Allon, 2017).

There was considerable overlap in the psychological principles underlying the therapeutic modalities employed within the included studies. Almost all included a psychoeducational component, if this was not offered as a standalone treatment as in 12.5% of Phase 1 studies. Many studies shared aspects of behavioural modification or adaptive coping skills training within Cognitive Behavioural Therapy, Cognitive Therapy (CT), Cognitive Processing Therapy (CBT), or Cognitive Experiential Therapy (CET) frameworks.

Six studies (25%) employed a CBT approach with all but one resulting in significant reduction of trauma symptoms with large effect sizes. Studies with a similar focus on cognitive processes and reprocessing or re-experiencing the traumatic event(s), either through CPT (2 studies, 8.3%) or CET (1 study, 4.2%) also elicited good outcomes with large effect sizes. Two studies (8.3%) had specific focus on relational aspects of trauma work using an IPT model. Although this treatment model engages with traumatic experiences in a different way, both studies demonstrated significant reductions in trauma symptoms with large effect sizes.

As might be expected, interventions that focused on only one aspect of treatment elicited smaller changes in participants' symptoms. Studies that included only psychoeducational material (5 studies, 20.8%) all demonstrated reductions in trauma symptoms, but effect sizes were smaller in magnitude, even when a large effect size

was achieved. Similarly, interventions that included only a cognitive aspect (1 study, 4.2%) did reduce trauma symptoms, but with a smaller effect size.

Quality Assessment

Table 3 summarises the methodological quality of each study as determined by the adapted AHRQ tool. There was considerable variance in the methodological quality of the included studies as determined by the adapted AHRQ tool. As noted above, a mean score was awarded as a numerical indicator of quality. For ease of summation, these scores are banded in to Very Low (0-0.5), Low (0.6-1.0), Fair (1.1-1.5), and Good (1.6-2). The majority of studies were of Fair (45.8%) or Good (25.0%) methodological quality, with 20.8% rated as Low and 8.4% as Very Low.

Despite the percentage of studies receiving a Fair or Good quality rating, there were some areas of assessment in which a high number of studies performed poorly. Only 37.5% of studies provided enough information regarding recruitment strategy to determine that there had been an unbiased selection of participants. *A priori* calculation of sample size was not completed in 87.5% of studies, and in 45.8% of studies missing or low data was either inadequately managed or insufficient information was provided to assess how authors had approached this issue. In 41.7% of studies the authors were rated as inadequately addressing study limitations, either by not identifying key limitations identified by the raters, or not appropriately addressing how limitations could have been avoided or should be addressed in future research.

Discussion

Summary of Results

The aim of the review was to assess the evidence provided for group-based treatments for individuals who have experienced interpersonal trauma. Recent evidence in this area has focused only on RCTs (Mahoney et al., 2019) and though valuable, it does not capture the smaller clinical trials in which RCT protocol is not viable. These smaller trials may provide important indicators of what is working well within day-to-day clinical work, thus guiding future larger-scale trials.

Findings of this review indicate that group treatments were effective in reducing trauma symptoms and that these reductions were comparable – if not better – than control conditions, where a control was used. This is particularly important in considering the need for evidence-based treatment recommendations for the new ICD-11 (WHO, 2018) diagnostic category for CPTSD, which would include interpersonal trauma. Furthermore, as services nationally face ongoing challenges of austerity, evidence-based group treatments can maximise therapeutic provision in a cost-effective manner without diminishing quality of care.

There were variations in magnitude of effect size both within and between phases of treatment. The range of effect size in Phase 1 studies was greater than the range in Phase 1 and 2. Phase 1 interventions produced effect sizes ranging from small to large, though a majority of studies which provided sufficient data for calculation produced a medium effect size. In comparison, within Phase 1 and 2 studies all but one study produced a large effect size. These large effect sizes tended to be of greater magnitude than the more conservative effect sizes found in Phase 1-only studies.

Table 3.

Assessment of overall study quality using an adapted form of the Agency for Healthcare Research and Quality (AHRQ) assessment tool (Williams et al., 2010) of the included studies (*n*=24).

Study	Unbiased Selection of Cohort	Appropriate Comparison Group	A Priori Sample Size Calculations	Adequate Description of Cohort	Validated Method of Ascertaining Trauma Symptoms	Adequate Description of Group Intervention	Analyses Appropriately Assess Efficacy Intervention	Analyses Control for Confounding Variables	Missing or Low Data Adequately Handled	Limitations Identified and Adequately Addressed	Mean Score (max. 2)
Allon (2015)	No	Yes	No	No	Yes	Partial	No	No	Unclear	No	**0.5**
Ball et al. (2013)	Partial	N/A	No	Partial	Partial	Yes	Yes	No	Yes	Yes	**1.2**
Bass et al. (2013)	No	Partial	Partial	Yes	Yes	No	Partial	No	Yes	No	**0.9**
Brown et al. (2013)	Partial	N/A	No	No	Yes	Partial	Partial	No	Unclear	Yes	**0.8**
Chard (2005)	Yes	Partial	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	**1.7**
Classen et al. (2011)	Partial	Yes	No	Yes	Yes	Yes	Yes	Partial	Yes	No	**1.4**
Crespo & Arinero (2010)	Yes	Yes	No	Yes	Yes	Partial	Yes	No	No	Partial	**1.2**
Dorrepaal et al. (2012)	Yes	Partial	Yes	Partial	Yes	Yes	Yes	Yes	Yes	Yes	**1.8**
Echeburúa et al. (2014)	Yes	Yes	No	Partial	Partial	Yes	Yes	Partial	No	Partial	**1.2**
Empson et al. (2017)	Partial	N/A	No	Partial	Yes	Yes	Partial	Yes	No	Yes	**1.2**
Hien et al. (2009)	Yes	Partial	No	Yes	Partial	Partial	Partial	Partial	Yes	Yes	**1.2**
Karatzias et al. (2016)	Yes	N/A	No	Yes	Partial	Yes	Yes	Yes	Yes	Yes	**1.7**

Kaslow et al. (2010)	Yes	Yes	Yes	Yes	Partial	Yes	Yes	Yes	Yes	Yes	**1.9**
Katz (2016)	Unclear	N/A	No	Partial	Yes	Yes	Yes	Partial	Unclear	Yes	**1.1**
Kelly & Pich (2014)	Partial	N/A	No	Partial	Partial	Partial	Partial	No	Unclear	Partial	**0.7**
Krupnick et al. (2008)	Partial	Partial	No	Yes	Yes	Yes	Partial	Yes	Yes	Yes	**1.5**
Lubin et al. (2009)	Partial	N/A	No	Partial	Yes	Yes	Yes	Yes	Unclear	No	**1.1**
Messina et al. (2012)	No	Partial	No	Yes	Yes	No	Yes	No	Yes	No	**0.9**
Mueser et al. (2007)	Unclear	N/A	No	Yes	Partial	Partial	Partial	Partial	Yes	Yes	**1.1**
Sayin et al. (2013)	Partial	N/A	No	Yes	Yes	Partial	Partial	Unclear	Yes	No	**1.0**
Sikkema et al. (2004)	Yes	N/A	No	Yes	Yes	Yes	Yes	Yes	Unclear	Yes	**1.6**
Sikkema et al. (2007)	Yes	Partial	No	Yes	Yes	Partial	Yes	Yes	Yes	Yes	**1.6**
Wallis (2002)	No	Partial	No	No	Yes	Partial	Partial	Unclear	Unclear	No	**0.5**
Zlotnick et al. (1997)	No	Partial	No	Yes	Yes	Partial	Partial	Partial	Partial	Yes	**1.1**

Note. Mean score calculated by awarding points for each criteria – 'Yes'=2 points, 'Partial'=1 point, 'No' and 'Unclear'=0 points. These are then averaged by the number of relevant criteria (i.e. nine in the case where there was no control group, or ten if a control group was present).

However, this does not devalue the impact of Phase 1-only treatment. It is to be expected that by progressing further in treatment, participants in Phase 1 and 2 studies may exhibit a greater reduction in symptoms. By including quasi-experimental and observational studies, this review has been able to evidence that Phase 1 intervention alone can reduce trauma symptoms significantly, with further reductions when Phase 2 work is introduced. This offers a somewhat different perspective to conclusions drawn by Mahoney et al. (2019), whose meta-analysis of RCTs suggested Phase 1 interventions may be beneficial for reducing general distress, but that Phase 2 treatment was required for reduction of symptoms of traumatic stress.

Given the heterogeneity of therapeutic modalities employed and the overlap of psychological principles underpinning them, it is difficult to make meaningful comparisons of efficacy between different models. Any conclusions must be made tentatively and in the context of the limitations of the evidence. There is a small trend to greater reductions in trauma symptoms with larger effect sizes in studies featuring an aspect of re-experiencing or re-enacting as a component of reprocessing the traumatic memories (e.g. CPT, CET). Interventions that included a relational component (i.e. IPT) also elicited good outcomes with large effect sizes.

In consideration of informing clinical practice, this review highlights that short-term group interventions can still be effective in reducing symptoms, with standalone psychoeducational treatments resulting in significant reduction of trauma symptoms. Longer-term group treatments that incorporate Phase 2 work require further consideration as to the skill set of practitioners delivering therapy. This is particularly important for therapeutic modalities where accreditation may be required (e.g. IPT), or where there is greater focus on re-experiencing, which may

require a greater degree of experience to provide adequate containment throughout the treatment process.

Limitations of Studies

Models of group treatment are heterogeneous though all tend to fall into phase-based categories, with the content of the group therapy matched to one or more phases of treatment. This is in-keeping with existing guidance and recommendations for treatment (e.g. NES, 2015). However, the lack of clarity regarding group implementation in some studies which reported using 'eclectic' or 'flexible' approaches (e.g. Kelly & Pich, 2014; Sayin et al., 2013) creates a number of issues in terms of comparing or replicating interventions.

The issue of replicability is also significant where individual therapy for trauma is used alongside the group intervention, or as a control condition. In all but one study (Echeburúa et al., 2014) there was minimal detail of the content, duration, or purpose of the individual work, and as such it is impossible to determine the extent to which positive findings are due to the group interventions being assessed. Where group therapies are used as an adjunct to individual treatment (e.g. Brown et al., 2013), treatment control conditions using individual treatment only must be included to aid determination of additional benefits derived from the group component.

General methodological issues also limit the reliability and validity of findings. A large majority of studies used female-only samples, and even in studies where males were included they were under-represented. There is clear evidence men are also victims of interpersonal trauma, particularly physical abuse (Briere & Elliott, 2003). Under-reporting of interpersonal trauma, particularly trauma of a sexual nature, is

common in men (Javid, 2016; Lowe & Rogers, 2017) and we must be conscious of our role in perpetuating this through the lack of inclusion in research.

Additionally, recruitment strategies tended to be poor in quality with insufficient information to determine method of recruitment or elimination of bias. There was a large proportion of studies with small (n < 50) samples, or with very specific samples (e.g. Empson et al., 2017; Sikkema et al., 2004; Sikkema et al., 2007), which limits the extent to which these can be applied to general clinical populations. A large number of studies also excluded individuals who reported any suicidal ideation or self-harm. This strongly biases any results given the extremely high correlation between experiences of interpersonal trauma and suicidal behaviour (e.g. Reuben et al., 2016; Stein et al., 2018, Turner et al., 2017), and as such findings must be interpreted with caution.

Strengths and Limitations of this Review

This review benefits from a search strategy that aimed to reduce publication bias with the registration of an *a priori* review protocol reducing risk of duplication and improving its overall rigour. The high level of inter-rater reliability for methodological quality suggests both that the adapted AHRQ tool worked well in this setting and that these ratings are accurate. However, meta-analysis could not be conducted due to the heterogeneity of group intervention types, methodologies employed, and analyses conducted (Centre for Reviews and Dissemination; CRD, 2009).

This review benefits from the inclusion of small-N studies. While under-powered, these studies provide insights into what works for whom and potentially

informs mechanisms of change within clinical settings. This is particularly important in cases where it is unfeasible or unethical to include No-Treatment control conditions due to staff or service capacity. Therefore, while the sample included in this review lacked sufficient homogeneity in methodological approach to conduct a meta-analysis, one of its strengths lies in the clinical utility of its findings.

Despite the valuable findings of this review, there are some limitations that should be considered when interpreting the results. Inclusion criteria were revised to focus only on outpatient group treatments given the possible confound of additional support received in inpatient or forensic settings. However, it would be useful for future research to consider studies conducted within these settings, and to compare with outpatient group outcomes, to determine whether the structure and additional support of inpatient and forensic units results in significant differences between patient environments. Furthermore, while interpersonal trauma is a large component of the types of trauma characterised within ICD-11 CPTSD diagnostic criteria (WHO, 2018), this review has not considered other traumas that may be captured by this diagnosis. As such, caution must be applied if generalising these findings to treatment of CPTSD where this includes other types of traumatic experience.

Implications for Research

The findings suggest a clear need for methodological improvements in assessing effectiveness of group interventions for interpersonal trauma. More transparent and unbiased recruitment strategies are needed, and future research should provide greater detail when describing both sample characteristics and the process of the

intervention. As noted, many of the included studies use small sample sizes, and just over half included any follow-up assessment of treatment outcomes. In many cases these studies are piloting new group treatments, or using existing group treatments in different settings, and their methods are limited accordingly. However, it is important for these studies to be built upon and group treatments employed in larger, higher quality research that measures treatment outcomes in longer-term follow-ups.

None of the included studies describe any Phase 3 component of treatment. This may be due to this phase requiring a more tailored approach that is ill-suited to group treatment, or that this was achieved through a separate intervention. Where Phase 3 is not included within the therapeutic intervention, it would be useful for authors to acknowledge how participants were supported to engage in this phase of treatment to provide greater clarity around the patient journey and to better inform other researchers and practitioners of considerations required if implementing a similar approach.

The number of studies excluded due to insufficient assessment of treatment efficacy is concerning given the minimal requirement of pre- and post-treatment assessment with one or more outcome measures evaluating trauma symptoms, informed by research guidance by Moore et al. (2015). A high proportion of studies excluded did not gather any post-treatment data or did not use a trauma outcome measure despite delivering a treatment for interpersonal trauma. Although the focus of some studies being group process rather than group outcome may account for a small number of exclusions, there is still a high proportion of treatment outcome studies that inappropriately assess efficacy. A further methodological issue is the lack of *a priori* sample size calculations in the majority of included studies. This may lead

to misinterpretation of large effect sizes that are due in part to the small sample size, rather than wholly an indication of treatment effectiveness. In agreement with Bakker et al. (2016), this review demonstrates the need for formal power calculations to be documented, as researchers' intuition or prior practice are insufficient methods of determining power.

Only quantitative evidence was considered in this review, and future research may benefit from considering studies with a mixed-methods or qualitative design. Rodgers and Elliott (2015) note that qualitative data can be invaluable in determining which components of an intervention helped to facilitate change, and it is a useful way to engage patients as stakeholders in ongoing service evaluation and development. Gathering qualitative data regarding participants' perceptions of group treatment is important to ensure interventions are not delivered in a manner presumed to be most beneficial or desirable (e.g. single gender) without supportive evidence.

It is worth noting that some studies that included a control condition used a Waiting List, Delayed Treatment, or Minimal Contact control. Whilst the challenges of using a treatment control are appreciable, studies that compare group treatment outcomes to No or Minimal Contact controls are evidencing only that some treatment is better than no treatment. Mohr et al. (2009) discuss the possibilities for inflated effect sizes when No or Minimal Treatment control conditions are used, which poses significant risk of bias when comparing evidence for different interventions. Future research should aim to include comparable treatment controls wherever possible, with sample sizes high enough to draw meaningful comparisons between experimental and control condition

Finally, it should be noted that many included studies were conducted in the US, or other countries outwith the UK. While this does not diminish the findings of these studies, there are significant differences in healthcare between the UK and countries that have largely privatised healthcare provision, which may impact the range of individuals accessing treatment. Nationalised provision of healthcare in the UK results in considerable differences in service structure, capacity, and demand when compared to privatised healthcare. It would therefore be beneficial for more research to be carried out within the UK in order to understand any differences in methodological approach or treatment outcome as a result of the different healthcare provision.

Implications for Clinical Practice

This review clearly demonstrates that group treatments are important in treating symptoms relating to interpersonal trauma. Either employed independently or as an adjunct to ongoing treatment, evidence presented here suggests the use of group-based therapy allows a higher volume of patients to receive treatment that is more effective than prolonged periods on waiting lists, and comparable to individual treatment. It may be that this is offered for Phase 1 only, where there are often ancillary reductions in secondary symptoms, with the more intense reprocessing work continued in individual treatment or with an individual-plus-group format (e.g. Chard, 2005; Echeburúa et al., 2014). Regardless, there is a strong economic argument for the use of group interventions for all or part of trauma treatment.

It should also be noted that the evidence within this review focuses specifically on amelioration of trauma symptoms. Although Mahoney et al. (2019) concluded

Phase 2 interventions elicited the most promising results in reducing symptoms associated with interpersonal trauma, their meta-analysis considered effects of treatment on symptoms of PTSD, depression, general psychological distress, substance misuse, and dissociation. This, in addition to the inclusion of non-RCT studies, may account for the different findings around efficacy of Phase 1-only interventions.

While often based on similar psychological principles, the range of treatments employed in the included studies makes it difficult to provide specific recommendations. This suggests it may be beneficial for practitioners to collaborate more closely with research colleagues to support the dissemination of service level research, and to ensure that promising pilot studies progress to more rigorous clinical evaluations.

Conclusions

This review offers support for the benefit of group therapies in Phases 1 and 2 of treatment for symptoms associated with interpersonal trauma, with great value as an adjunct to ongoing treatment and some tentative evidence supporting its use as a standalone intervention. By broadening the inclusion criteria beyond RCTs, additional evidence has been gathered to that documented within Mahoney et al. (2019), and this suggests there is measured efficacy of Phase 1 interventions in addition to the Phase 2 interventions identified within their meta-analysis. It is suggested that group interventions provide a means of increasing access to psychological services in an economical way. However, this review also highlights the need for continuing research in this area that employs a greater degree of

methodological rigour to ensure that the evidence produced is of higher quality. Qualitative research may help to elicit better understanding of the group therapy mechanisms that facilitate change. Furthermore, research based within the UK is required to determine whether evidence gathered overseas can be applied within a nationalised, rather than privatised, healthcare setting. Overall, this systematic review provides good evidence for the implementation of a phase-based approach in the treatment of symptoms associated with interpersonal trauma, which can help to guide local and national clinical and policy decision-making.

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Journal Article 2: Empirical Study[a]

Title: The role of emotion regulation and social connectedness in the relationship between childhood interpersonal trauma and suicidal thinking.

Short title: *Childhood Trauma and Suicidal Thinking*

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Abstract

Objectives: The current study aimed to examine how emotion regulation and social connectedness may impact upon the relationship between childhood abuse and suicidal thinking (defined as suicidal ideation or intent).

Methods: Data were collected from patients receiving group treatment for interpersonal trauma within an outpatient psychological therapies service. Of the 533 patients referred during the data collection period, 242 attended at least one session (291 refused treatment). Of these, 106 patients (96.2% female) provided complete pre-treatment data and this sample was used for cross-sectional bivariate correlation and mediation/moderation analyses.

Results: Emotion regulation directly mediated the relationship between childhood emotional and physical abuse and suicidal thinking. Family identification mediated the association between physical abuse and suicidal thinking, but not the association between emotional abuse and suicidal thinking. Identification with a group of one's choice mediated the impact of physical abuse. For childhood sexual abuse, emotion regulation and family identification moderated the abuse-suicidal thinking relationship.

Conclusions: Promoting adaptive emotion regulation and improving social connectedness can reduce suicide risk and the negative impact of childhood abuse. Participants had to disclose interpersonal trauma to be referred, which fails to capture those who are unable or do not wish to disclose, or who are not in contact with services. Further research using more generalised samples is required. However, the sample is an accurate representation of the population presenting to mental health services, and as such these findings have clear clinical relevance.

Keywords: childhood trauma, interpersonal trauma, child abuse, suicidal ideation, suicidal intent, emotion regulation, social connectedness

Practitioner Points

- Emotion regulation can inform interventions to reduce suicidal thinking
- Strong bonds with one's family and friendship groups are important protective factors in reducing the suicidal thinking associated with childhood abuse
- Promoting development and maintenance of interpersonal relationships may serve as a beneficial and cost-effective adjunct to psychological treatment
- Further research is required to understand how family identification interacts with the relationship between childhood abuse and suicidal thinking
- Findings should be considered in the context of a trauma-specific sample who have a high baseline of adversity, which may limit their generalisability

Word Count: 4995 words

Introduction

Despite progress in recent years globally there are 800,000 deaths by completed suicide each year, with the World Health Organisation (WHO) endorsing steps to reduce the suicide rate by 10% by 2020 (WHO, 2018). Therefore, the reduction of factors associated with elevated suicide risk are a key focus of governmental policies worldwide.

Evidence suggests those who have experienced interpersonal trauma – where intentional harm is done to the individual by another person, either by abuse or neglect – are significantly more likely to exhibit suicidal behaviour (ideation, intent, or attempt) than peers who have not had traumatic experiences (e.g. Reuben et al., 2016), or compared to individuals who have experienced other types of trauma, such as natural disaster or traffic accident (Yoo et al., 2018). Suicide risk is particularly heightened in those who have experienced childhood sexual abuse (CSA; Perez, Jennings, Piquero, & Baglivio, 2016) or other forms of childhood maltreatment (e.g. Stein et al., 2018).

One mechanism by which childhood interpersonal trauma increases risk could be via the impact upon development of adaptive emotion regulation skills, which would usually serve as a protective factor against suicidal behaviour (Barr, Fulginiti, Rhoades, & Rice, 2017; Xavier, Pinto-Gouveia, & Cunha, 2016). Emotion regulation strategies develop across childhood and adolescence but experiencing interpersonal trauma in these early years may disrupt development of adaptive strategies given the likely stressful environment and lack of appropriate modelling (e.g. Dunn, Nishimi, Gomez, Powers, & Bradley, 2018). Research demonstrates that emotion regulation

mediates the relationship between childhood trauma and adult psychopathology (e.g. Espeleta, Palasciano-Barton, & Messman-Moore, 2017; Moulton, Newman, Power, Swanson, & Day, 2015). Consequently, emotion regulation training is a key component of evidence-based trauma interventions (de Jongh et al., 2016).

Childhood interpersonal trauma often occurs in an environment that should be a place of safety for the child, and the lack of perceived safety has a profound impact on the child's ability to form healthy and enjoyable social relationships both within and outwith the family setting (Martin et al., 2016; Wilkinson, 2016), as well as their risk of suicidal thinking (van der Kolk, 2017). Shevlin, McElroy and Murphy (2015) demonstrated that loneliness mediated the relationship between childhood trauma and poor mental health in adulthood, with those who enjoyed a greater degree of social connectedness experiencing better mental health. A higher degree of social connectedness has repeatedly been shown to act as a protective factor against suicidal behaviour (e.g. Gunn III, Goldstein, & Gager, 2018; Taliaferro & Muehlenkamp, 2017).

Aims and Hypotheses

The current study aimed to examine the relationship between childhood interpersonal trauma and suicidal thinking, and to consider the role emotion regulation and social connectedness may play within that relationship. The study used data gathered in a specialised trauma service which delivers a psychoeducational safety and stabilisation treatment in a group-based setting for individuals who have experienced interpersonal trauma. The service operates within outpatient psychological therapies but accepts referrals from all mental health services within the locality. The literature demonstrates positive associations

between childhood interpersonal trauma and suicidal behaviour, which includes suicidal ideation and intent, though research often focuses on one kind of trauma (e.g. CSA), so less is known about how the effects of different kinds of abuse experienced in childhood may compare within a theoretical model.

This study's primary aim was to investigate theoretical models (mediation/moderation) that explore the links between childhood trauma and suicidal thinking, and whether these might be impacted by emotion regulation and/or social connectedness. Evidence has suggested these factors have independent associations with both childhood trauma and suicidal thinking. There is good evidence that emotion regulation seems to impact the relationship between childhood trauma and later mental ill-health (e.g. Espeleta et al., 2017), with emerging evidence that social connectedness – or lack thereof – may play a similar role (Shevlin et al., 2015). However, no study to date has examined the impact of these variables together within the same theoretical model. It is hypothesised that emotion regulation skills and level of social connectedness will affect the relationship between childhood trauma and suicidal thinking and, if a causal impact is found, that better emotion regulation skills and a higher degree of social connectedness will result in reduced suicidal thinking.

Methods

Participants and Procedure

The current study used data from a specialised trauma service operating within an outpatient psychological therapies setting. Data were collected over a 26-month period between October 2016 and December 2018; the use of which was approved by

Information Governance and Research Ethics boards of both NHS Tayside and the University of Edinburgh. The service delivers group therapy for individuals who have experienced interpersonal trauma, using the Survive and Thrive (SaT) manualised programme (Ferguson, 2008; distributed by NHS Education for Scotland, NES). Based on Herman's (1992) three-phase treatment model for interpersonal trauma, SaT aims to achieve safety by reducing maladaptive coping strategies and aiding the stabilisation of mental health via a 10-week psychoeducational group treatment.

Each two-hour session is didactic in nature and discourages self-disclosure, though there is opportunity for discussion around the content of the session. SaT covers the psychological and physiological effects of trauma, with sessions devoted to common symptoms of trauma including anger, anxiety, depression, nightmares and flashbacks, to adaptive coping (e.g. grounding, relaxation exercises, mindfulness) and effective communicating. Groups are same-sex, accommodate up to 12 individuals and are facilitated by two or more clinicians. NES requires at least two clinicians with a mental health background and who have been trained in the SaT treatment to facilitate each group. Within the current sample, facilitators included Clinical and Counselling Psychologists and Clinical Associates in Applied Psychology (CAAPs), with Trainee or Assistant Psychologists observing in some groups as part of their training and development.

Participants in the study were individuals referred to the service by clinicians across a range of psychological and psychiatric services including primary care (n = 244, 45.8%), secondary outpatient care (n = 150, 28.1%), crisis services (n = 134, 25.1%), and substance misuse services (n = 5, 1.0%). To be referred for SaT an individual must have experienced interpersonal trauma in childhood and/or adulthood. Other SaT service inclusion criteria were possible suicidal behaviour (e.g. self-harm, expressed

suicidal ideation/intent, suicide attempt) and an age of 18 years or over (or over 16 years if the individual is no longer in full-time education). Individuals may be in mental health treatment already, except where this is individual psychological therapy for interpersonal trauma. Participants did not require a formal diagnosis of Post-Traumatic Stress Disorder (PTSD). Exclusion criteria included: current inpatient admission; deemed medically unfit to participate; unable to speak sufficient English to engage in treatment or who are not of an intellectual capacity that allows for participation in group-based treatment; or individuals who are known perpetrators of abuse.

Of the patients referred to SaT during the inclusion period (n = 533), 291 refused treatment by the SaT service, and 242 commenced treatment. Of these 242 patients, 136 did not complete some or all of the questions on one or more measures. Only scores from participants who had no missing data were included for the current study to prevent any effects of missing data from confounding results of testing the theoretical model. Therefore, the total sample size of the current study is N= 106 (3.8% male), with an age range of 20 to 67 years (M = 36.75, SD = 13.16). Demographic characteristics of the sample are shown in Table 1.

Table 1.

Demographic characteristics of the included sample (n=106)

Variable	N (%)
Relationship Status	
Single	52 (49.1)
Married	24 (22.6)
Separated	2 (1.9)
Divorced	8 (7.5)
Living with Partner	14 (13.2)
Widowed	4 (3.8)
Other	2 (1.9)
Employment Status	

Employed	51 (48.1)
Unemployed	39 (36.8)
Student	16 (15.1)
Retired	0 (0)
Diagnoses	
Bipolar Affective Disorder	0 (0)
Depression	60 (56.6)
Schizophrenia	1 (0.9)
Personality Disorder	5 (4.7)
Eating Disorder	5 (4.7)
Harmful Alcohol Use	7 (6.6)
Harmful Substance Use	0 (0)
OCD	2 (1.9)
Adjustment Disorder	0 (0)
Anxiety	15 (14.2)
PTSD	35 (33.0)
Other	20 (18.9)
No Diagnosis	18 (17.0)
Childhood Trauma Experienced	
Sexual Abuse	54 (50.9)
Physical Abuse	58 (54.7)
Emotional Neglect	56 (52.8)
Physical Neglect	37 (34.9)
Witnessing Violence	56 (52.8)
Adulthood Trauma Experienced	
Sexual Abuse	37 (34.9)
Domestic Abuse	58 (54.7)
Stalking	15 (14.2)
Harassment	22 (20.8)
Single Incident of Rape	39 (36.8)
Previous Psychiatric Input	60 (56.6)
Current Psychiatric Input	52 (49.1)

Measures

Traumatic experiences in childhood were assessed using the Childhood Trauma Questionnaire Short Form (CTQ; Bernstein et al., 2003). This is a 28-item self-report questionnaire that provides a history of childhood maltreatment across five domains: sexual (CSA; α = .98), physical (CPA; α = .87), and emotional abuse (CEA; α = .86), and physical (CPN; α = .83) and emotional neglect (CEN; α = .87). Items are presented in

statement form (e.g. "*I didn't have enough to eat"*) and participants are asked to rate statements on a five-point scale (1= 'Never True', 5 = 'Very Often True'). Evidence suggests this widely-used measure is robust with good internal consistency and suitable for use across a range of clinical populations (Spinhoven et al., 2014).

Evans et al.'s (2000) Clinical Outcome Routine Evaluation Outcome Measure (CORE-OM) is a 34-item self-report questionnaire designed to measure global psychological distress across four domains: well-being, social functioning, problems/symptoms, and risk (α = .94). Items are presented in statement form, e.g. "*I have felt unhappy*", and participants are asked to rate on a five-point scale how much they have experienced this over the last week (0 = 'Not at all', 4 = 'All of the time'). The CORE-OM is extensively used in research and clinical practice to monitor therapeutic gains and changes in presentation, and its validity has been tested rigorously (e.g. Connell et al., 2007; Evans et al., 2002). The subscale risk-to-self items of the CORE-OM, which provide a measure of participants' suicidal ideation and intent as well as thoughts of self-harm were used as the measure of suicidal thinking. These four items (α = .81) ask participants to rate the experiences of suicidal thoughts such as "*I have thought of hurting myself*" or "*I have made plans to end my life*". The reliability and validity of the risk subscale is well-documented in Evans et al.'s (2002) study, where it was shown to be comparable to risk subscales within other commonly used mental health screening tools, which was confirmed in further research by Leach et al. (2005).

Emotion regulation was measured using the Difficulties in Emotion Regulation Scale – Short Form (DERS-SF; Kaufman et al., 2016); a brief 18-item questionnaire designed to streamline the original DERS created by Gratz and Roemer (2004; α = .87). As in the DERS, participants are asked to indicate how often the presented statements are true for them on a five-point scale (0 = 'Almost never', 4 = 'Almost always').

Kaufman et al. (2016) tested the DERS-SF against the DERS across five different populations, including those that demonstrated suicidal behaviour, and found it maintained the high reliability of the original measure with half the items. Further research evidenced its psychometric properties in a range of clinical populations (Hallion, Steinman, Tolin, & Diefenbach, 2018).

The level of participants' social connectedness was assessed by measuring their identification with three social groups (Family, Community, and Other – a social group of choice) using the self-report Group Identification Scale questionnaire (GIS; Sani, Herrera, Wakefield, Boroch, & Gulyas, 2012). For the group of choice, participants select from a list including sports teams, work colleagues, friends, or religious groups, or they may indicate a group not included within this list. The GIS measures participants' sense of belonging to each group (e.g. "*I feel a bond with my [group]*") alongside the degree of commonality they share with other group members (e.g. "*I have a lot in common with the members of my [group]"*). Participants are asked to rate their agreement for each statement on a seven-point scale (1 = 'strongly disagree', 7 = 'strongly agree'), with the same four statements repeated for Family group (α = .88), Community group (α = .95), and Other group (α = .96). Research has shown the GIS has good reliability and accuracy when compared to existing well-validated measures (Sani et al., 2012; Sani, Madhok, Norbury, Dugard, & Wakefield, 2015), and it has since been used in large-scale clinical research examining associations between social connectedness and depression in an outpatient sample (Cientanni et al., 2017).

Analyses

Analyses were conducted using Statistical Package for Social Science (SPSS) software version 24 (IBM Corp., 2016). Following descriptive statistics, Pearson bivariate correlations were conducted to examine relationships between measures and to determine which should be entered into the regression model. Linear regression by block entry was used to explore predictors of suicidal thinking, before testing the theoretical models within mediation analysis. Where there were strong correlations between childhood abuse and suicidal thinking, but no predictors were apparent, moderation analysis was used to determine whether there were interactions present between these variables. Both moderation and mediation analysis were conducted using the PROCESS.v3 add-on for SPSS (Hayes, 2017). These analyses use a bootstrapping approach and do not require a specific sample size. However, Fritz and MacKinnon (2007) produced sample size guidance for researchers to achieve effect sizes of different magnitude to a power of 0.8. Their guidance indicates a minimum sample size of 71 is required to achieve a power of 0.8 with a medium effect size. The sample for the current study (N = 106) satisfies these requirements.

Results

Descriptive Statistics

Means and standard deviations across measures are shown in Table 2. Data were found to be within acceptable parameters for skewness and kurtosis, with full details provided in Appendix 5. Previously, the GIS outcome measure has been used to

provide a total number of groups identified with, where identification is defined as a mean score of five or more in a domain (e.g. Cientanni et al., 2017; Sani et al., 2012). To the authors' knowledge, this is the first example of the domains being examined individually, which allows for the identification of more nuanced effects of identification with different groups.

Table 2

Mean and Standard Deviation (S.D.) scores across measures.

Measure	Mean	S.D.
CEA	17.64	5.54
CPA	11.07	5.88
CSA	14.06	8.62
CEN	16.33	5.14
CPN	11.46	5.46
DERS-SF	60.57	12.19
GISF	4.14	1.54
GISC	3.05	1.33
GISO	4.25	1.88
CORE-OM	5.07	4.07

Note. CEA = childhood emotional abuse, CPA = childhood physical abuse, CSA = childhood sexual abuse, CEN = childhood emotional neglect, CPN = childhood physical neglect, DERS-SF = Difficulties in Emotion Regulation Scale – Short Form; GISF = Group Identification Scale; Family domain, GISC = Group Identification Scale; Community domain, GISO = Group Identification Scale; Other domain, CORE-OM = CORE-OM risk-to-self items score.

Bivariate Correlations and Linear Regression

Associations between variables were examined using Pearson correlations and are presented in Table 3.

Evidence suggests that when variables are correlated with a value equal or greater to 0.7 the risk of collinearity distorting the theoretical model and subsequently biasing conclusions is great (Dormann et al., 2013). The strength of the correlation between CEA and CEN ($r = 0.72$, $p < 0.001$) indicated this would likely be an

issue should both be included in further analyses. Given that of these two variables only CEA correlated with suicidal thinking, CEN was excluded from further analysis. The Community domain of the GIS did not significantly correlate with suicidal thinking and was therefore excluded from further analyses.

Table 3.

Pearson correlations between childhood trauma, emotion regulation, social connectedness, and suicidal thinking.

	CEA	CPA	CSA	CEN	CPN	DERS-SF	GISF	GISC	GISO	CORE-OM
CEA	-	.63**	.32**	.72**	.57**	.19	-.48**	-.37**	-.35**	.31**
CPA		-	.37**	.61**	.54**	.31**	-.40**	-.21*	-.22*	.26**
CSA			-	.27**	.25*	.31**	-.18	-.01	-.33**	.30**
CEN				-	.63**	.10	-.54**	-.21*	-.32**	.17
CPN					-	.22*	-.56**	-.11	-.36**	.28**
DERS-SF						-	-.05	-.25*	.04	.26**
GISF							-	-.03	.05	-.25**
GISC								-	.39**	-.18
GISO									-	-.23*
CORE-OM										-

Note. CEA = childhood emotional abuse, CPA = childhood physical abuse, CSA = childhood sexual abuse, CEN = childhood emotional neglect, CPN = childhood physical neglect, DERS-SF = Difficulties in Emotion Regulation Scale – Short Form, GISF = Group Identification Scale; Family domain, GISC = Group Identification Scale; Community domain, GISO = Group Identification Scale; Other domain, CORE-OM = CORE-OM risk-to-self items score.

* $p<0.05$

** $p<0.01$

Mediation and Moderation

An initial regression analysis demonstrated the model significantly predicted suicidal thinking ($F(4, 101) = 3.452$, $p < .01$), however emotion regulation emerged as the only significant predictor ($t = 1.992$, $p < .05$; see Appendix 5). Therefore, outcomes

of the Pearson correlations and evidence from existing literature were used to determine which domains of the CTQ to enter into theoretical models.

Evidence clearly suggests that childhood maltreatment rarely occurs in a vacuum, and it is unusual for one type of abuse to occur in isolation (van der Kolk, 2017). All types of childhood maltreatment negatively impact an individual's development (e.g. Trickett & McBride-Chang, 1995). However, there is evidence to suggest that the negative outcomes resulting from maltreatment that takes the form of acts of commission (e.g. CPA, CEA, CSA) vary to those that result from acts of omission (e.g. CEN, CPN). It has been suggested those who experience neglect experience a much greater degree of cognitive impairment (Hildyard & Wolfe, 2002; van Dam et al., 2015), with development of different neural regions being impacted depending on the type of maltreatment experienced (Teicher & Samson, 2016). Furthermore, evidence has suggested those who experience neglect are likely to exhibit a greater degree of internalising emotion regulation strategies and a higher level of social withdrawal and isolation than those who experienced abuse (Cecil, Viding, Fearon, Glaser, & McCrory, 2017; Hildyard & Wolfe, 2002). Cumulatively, this evidence suggests that the childhood trauma-suicidal thinking relationship may be different when that trauma takes the form of neglect rather than of abuse, and that emotion regulation and social connectedness may also interact with this relationship differently. In consideration of this, it was determined that drawing conclusions regarding the impact of neglectful childhood experiences on suicidal thinking from only one facet of childhood neglect captured within the CTQ would not accurately represent the impact of neglect, particularly within a theoretical model previously untested. Therefore, further analysis focused only on domains of abuse within the theoretical framework.

Each of the included childhood abuse domains were examined in a series of parallel mediation models. The CEA model (Figure 1) showed direct mediation of the relationship with suicidal thinking through emotion regulation (t = 2.604, p < 0.05, 95% BCa CI [0.019, 0.142]). Though there was no effect of Family group identification, Other group identification indirectly mediated the relationship (Figure 1), b = 0.067, 95% BCa CI [0.005, 0.160]. This would suggest that if an individual is able to develop adaptive emotion regulation skills despite early emotional abuse, their risk of suicidal ideation or intent is reduced. Having a chosen group with which one identifies may also serve as a protective factor.

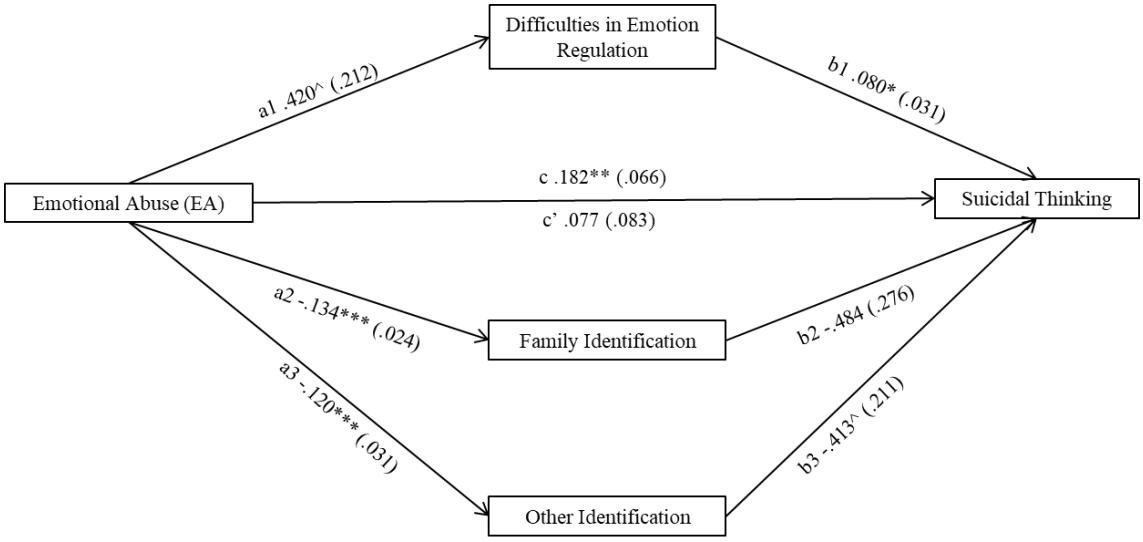

Figure 1. The mediation model of CEA and suicidal thinking, with coefficients and standard error (in brackets) shown for each path, where $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$, $^{\wedge}p = 0.05$.

In the CPA model (Figure 2), emotion regulation again acted as a direct mediator between abuse and suicidal thinking ($t = 2.531$, $p < 0.05$, 95% BCa CI [0.018, 0.145]), with more adaptive emotion regulation associating with reduced suicidal thinking. Social connectedness was also an important factor in the CPA-suicidal thinking relationship. Identification with Family group ($t = -2.122$, $p < 0.05$, 95% BCa CI [-1.075, -0.036]) and Other group of choice ($t = -2.299$, $p < 0.05$, 95% BCa CI [-0.863, -0.063]) directly mediated the relationship, suggesting that stronger identifications with these groups associated with reduced suicidal thinking.

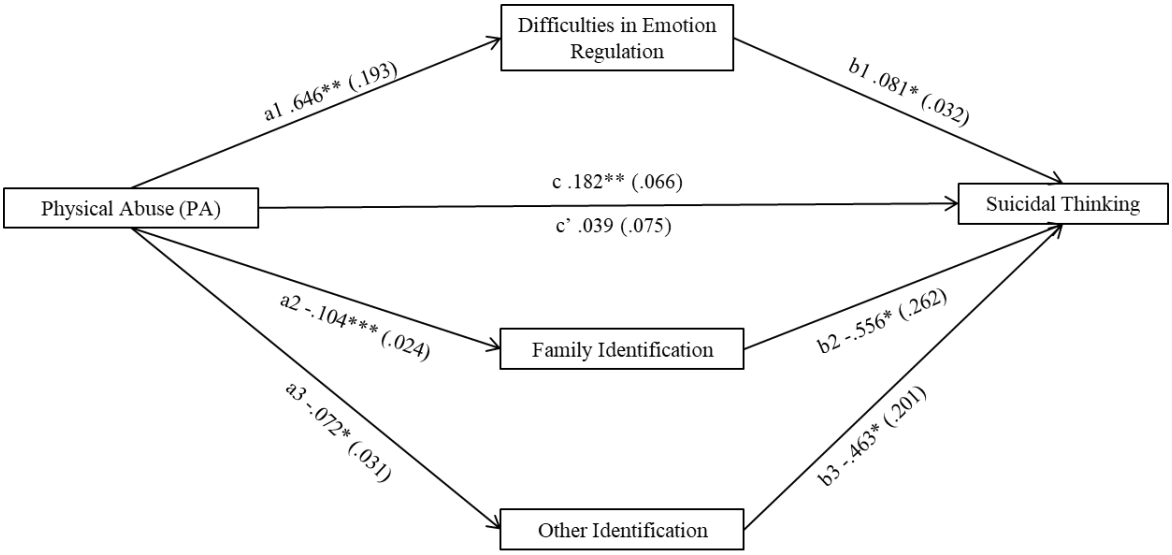

Figure 2. The mediation model of CPA and suicidal thinking, with coefficients and standard error (in brackets) shown for each path, where $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$, $^{\wedge}p = 0.05$.

In the CSA model there was a clear effect of the proposed mediators on the relationship with suicidal thinking, particularly emotion regulation and Family identification, however scores in the confidence intervals all crossed zero. This suggested that rather than a mediatory relationship between CSA and suicidal thinking, they may instead moderate the relationship. Testing of these patterns of association using a parallel moderation model confirmed this to be the case (Table 4). Analysis showed no moderation effect of Other group identification ($p = 0.17$), and so this is not included in the Table and Figures below.

Table 4.

Linear model of predictors of suicidal thinking.

	b	*SE B*	*t*	*p*
Constant	5.240 [4.510, 5.969]	0.368	14.250	< 0.001
CSA (centred)	0.100 [0.014, 0.186]	0.043	2.306	< 0.05
Family Identification (centred)	-0.561 [-1.021, -0.101]	0.232	-2.419	< 0.05
CSA x Family	-0.066 [-0.121, -0.012]	0.028	-2.405	< 0.05
Emotion Regulation (centred)	0.071 [0.011, 0.132]	0.031	2.331	< 0.05
CSA x Emotion Regulation	-0.010 [-0.017, -0.004]	0.003	-3.045	< 0.01

Note. CSA = Childhood Sexual Abuse.

The moderation analysis showed that better emotion regulation skills reduced the impact of CSA on suicidal thinking when there is a lower CSA score on the CTQ, but as the score on the CTQ for CSA increases, emotion regulation had less of an ameliorative effect on suicidal thinking (Figure 3). When CSA scores were low, there was little effect of identification with family (Figure 4). However, as CSA scores increased, lower identification with family associated with greater suicidal thinking than for those who identify strongly with their family group.

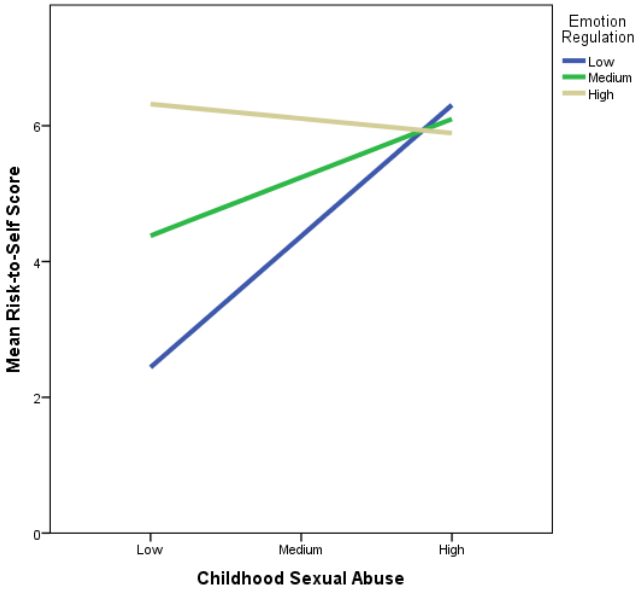

Figure 3. Moderator effect of emotion regulation on the relationship between childhood sexual abuse and suicidal thinking measured by CORE-OM risk-to-self items.

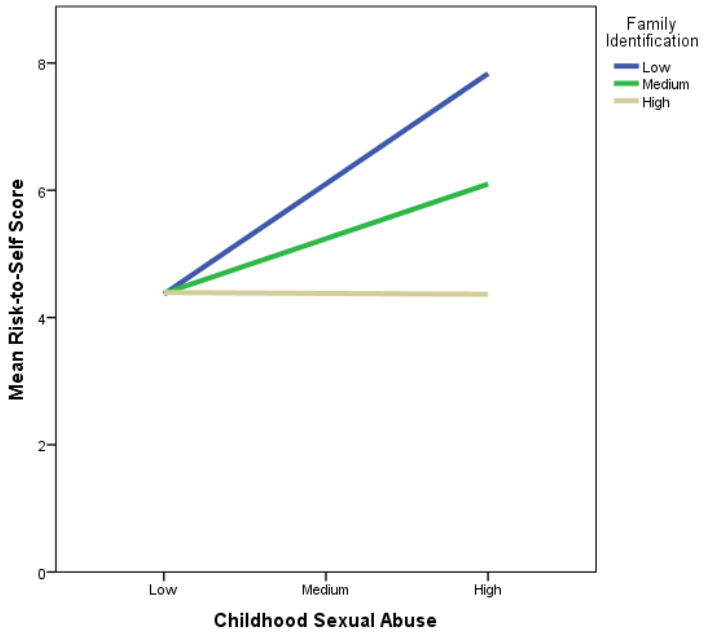

Figure 4. Moderator effect of family identification on the relationship between childhood sexual abuse and suicidal thinking measured by CORE-OM risk-to-self items.

Discussion

This study provides further evidence for the protective role of adaptive emotion regulation skills in those who have experienced childhood abuse. It is the first study of its kind to explore the role of social identification across different groups within a trauma population and provides evidence for the proposition that although one's family identification can play a role in mediating impact of childhood abuse on suicidal ideation and intent, it is the groups that one chooses that offer more of a buffering effect against suicidal thinking.

While there is a significant body of evidence demonstrating the impact of childhood trauma on the development of adaptive emotion regulation skills, and of poor emotion regulation on suicidal thinking, the evidence within the context of interpersonal trauma is more limited. Interpersonal trauma impacts an individual's development and prognosis in a manner that is far more systemic and entrenched than one might see following Type 1 trauma, given the often repetitive and inescapable forms these types of abuse and/or maltreatment take (Cook et al., 2005). Although they may meet diagnostic criteria for PTSD, evidence suggests these individuals will exhibit greater functional impairment (Brewin et al., 2017). This is reflected in the International Classification of Diseases, 11th edition (WHO, 2018), where Complex PTSD (CPTSD; i.e. trauma resulting from interpersonal harm over a prolonged period, often including early childhood) is recognised as distinct from PTSD due to the pervasive resulting difficulties in affect regulation and in sustaining interpersonal relationships.

Herman's (1992) three-phase treatment model for trauma promotes an initial phase of achieving safety from harm (from self or others) and a stabilisation of symptoms. Many treatments use psychoeducation and affect management training

at this stage to help individuals adapt from relying on often harmful emotion regulation strategies that provide short-term relief but are ineffective in the long-term, to those which are more adaptive (Boden et al., 2013). Although some have posited the evidence for a phased-based approach for interpersonal trauma is weak (e.g. De Jongh et al., 2016), a survey of trauma experts by Cloitre et al. (2011) found an overwhelming majority endorsed this type of approach. Without sufficient evidence to make definitive recommendations, local government guidance indicates phase-based treatments should be used for complex trauma (NES, 2015). Certainly, it has been demonstrated that training in adaptive emotion regulation strategies can benefit those who have experienced interpersonal trauma, reducing trauma symptoms and general distress (Karatzias et al., 2018). The current study demonstrates the important role emotion regulation skills can have in reducing suicidal thinking when childhood interpersonal trauma has been experienced.

The role of social connectedness appears to be similarly important in protecting against risk. Traditionally, research has examined this as a holistic entity with evidence from a number of studies indicating that social support is important in reducing psychological distress and disorder symptomatology (e.g. Cientanni et al., 2017; Shevlin et al., 2015). However, as adult attachment theory is increasingly being considered within the interpersonal trauma framework (Murphy, Elklitm Hyland, & Shevlin, 2016), it becomes more important to go beyond generalities and explore *who* a social connection with leads to improved outcomes.

In the current study domains of group identification were examined as separate entities, rather than using the measure to produce a total number of groups identified with. This is important when considering the impact a varying sense of belonging and identification with different groups may have for people who have experienced

interpersonal trauma, as it may differ from broader clinical samples or the general population. For example, understanding how family identification is affected for an individual who experienced childhood abuse within the family environment provides greater insight into the positive or negative effects of this specific social group. Considering the number of groups identified with as a whole does not give the opportunity for the same level of understanding.

In the current study, protective benefits of identification with family were found in models of CSA and CPA, but not in CEA. This is in-keeping with evidence that demonstrates the benefits of family identification on general health and some aspects of mental health (Alvarez, Kawachi, & Romani, 2017; Wakefield, Sani, Herrera, Khan, & Dugard, 2015). However, the role of family within childhood abuse may not always be protective. If a child is abused by their parents or main caregivers, distancing themselves from their family may be a better option for survival. Alternatively, if the abuser is a relative the individual may feel shame or a sense of guilt at disrupting familial relationships should they disclose (e.g. Lemaigre, Taylor, & Gittoes, 2017).

Family identification was not found to mediate the relationship between CEA and suicidal thinking, which correlates with findings suggesting that when comparing different types of childhood maltreatment, emotional abuse is the main predictor of later mental illness, over all other maltreatment types (Cecil et al., 2017). Considering this within the context of attachment, when the caregiver is the source of both safety and threat simultaneously, the child is never able to attain resolution of their fear. Evidence is clear that this would impact emotion regulation (e.g. Tatnell, Hasking, Newman, Taffe, & Martin, 2017) but in particular it would affect the child's ability to form trusting, healthy relationships throughout life. The experience of emotional abuse in childhood can lead to the belief that relationships are a source of

fear and uncertainty, with recent research suggesting the lasting effects on adult attachment styles are key in understanding suicidal ideation in adulthood (Allbaugh et al., 2018).

It appears that within the domain of social relationships, the ability to form bonds with a group of one's choosing, regardless of the nature of that group, is the greatest protector against suicidal behaviour. As Shevlin et al. (2015) demonstrated, loneliness is a significant mediator between childhood trauma and adult psychopathology. There is clear evidence that friendships act as protective factors against development of trauma symptoms and against suicidal thinking (e.g. Marver et al., 2017), and the current study suggests friendships can ameliorate the negative impact of childhood abuse.

In terms of clinical utility, these findings could highlight possible adjuncts to existing treatments as a means of both increasing access to services and improving treatment efficacy. When services are becoming increasingly stretched, utilising low-cost but high-impact interventions can help individuals achieve safety and stabilisation perhaps even prior to contact with mental health services. The use of social prescribing is gaining popularity in the UK, with evidence suggesting it may be a useful addition to ongoing therapies or as pre-treatment support (e.g. Wildman et al., 2019). Alternatively, the use of group treatments for trauma, such as the one employed within the current study, can provide a sense of belonging with individuals with shared experience; a powerful contributor to improving treatment attendance and outcomes (e.g. Katz, 2016).

Limitations

It is important to consider the findings of this study within the context of its limitations. The variance in suicidal thinking accounted for by the model is relatively small. This is to be expected given the known extensive range of variables that can affect rates of suicide, which includes demographics (Huang, Ribeiro, Musacchio, & Franklin, 2017), life stressors (Buitron et al., 2016; Lund, Nadorff, Winer, & Seader, 2016), seasonal and weather changes (Aguglia et al., 2019; Moore et al., 2018), in addition to general health and wellbeing (Barak-Corren et al., 2016). Therefore, the role of emotion regulation and social connectedness in mediating or moderating the relationship between childhood trauma and suicidal thinking is just a small part of the overall picture. They provide indications of important directions in clinical intervention, but other known factors that impact this relationship – or suicidal behaviour more broadly – must be considered when formulating individuals' risk of suicide.

Furthermore, while the CORE-OM risk subscale is well-validated, it may be useful for future research to consider the inclusion of a measure of suicidal behaviour or risk of repetition in addition to suicidal cognitions addressed within the CORE-OM. This is one of the difficulties in using service-level routinely collected data, where research utility must be weighed against patient burden. In this case, it was deemed inappropriate to add further measures to the battery of assessment undergone by patients within the SaT service. Future research may consider the use of a more detailed suicidal thinking or suicidal behaviour measure within this theoretical framework, which would have been the preferred manner to assess this within the current study. However, it should be noted that the regular use of the CORE-OM in clinical settings to assess an individual's risk-to-self indicates its acceptability to

clinicians for accurately identifying and measuring suicidal ideation. It may be that when study design is constrained by service requirements, capturing risk-to-self via the CORE-OM Risk sub-scale may be a viable method of assessing suicidal thinking in a manner that minimises burden to participants but provides data deemed adequate by clinicians for monitoring risk.

The generalisability of this study's findings is perhaps limited by the size and specificity of the sample, as well as the cross-sectional design. While providing a useful snapshot, the cross-sectional design can only allow inferences to be made about the point at which data were gathered. To truly measure ameliorative effects of adaptive emotion regulation skills and greater social connectedness, a longitudinal design is required. This would be a useful avenue for further research and would help to greater evidence efficacy of the intervention. The smaller sample size within the current study prevented the inclusion of control variables within the theoretical models, as this would have caused them to become under-powered. While not uncommon in studies of this type, in the interests of further testing of these theoretical models it would be useful for future research to be adequately powered so as to include control variables within the mediation and/or moderation analyses.

Finally, while evidence suggests that the prevalence of interpersonal trauma is high (NSPCC, 2019), that an individual must have disclosed interpersonal trauma to be referred to the SaT service may suggest factors not accounted for in this model. In a recent systematic review Lemaigre et al. (2017) highlighted the considerable number of barriers to disclosing sexual abuse, including feelings of shame, guilt, or self-blame, and perceived negative consequences or limited support. To overcome such barriers to disclose interpersonal trauma may require a resilience or process of change not captured here, and which demarcates the current study's sample from a more general

one. It is true that having a sample of individuals who have presented to mental health services is more representative of those experiencing these types of difficulties in day-to-day life, the ecological validity of this study could be further improved by aiming to include those who may struggle to access conventional services due to the complex needs that often present alongside a history of interpersonal trauma.

Conclusions

Despite these limitations, this study has a number of strengths. Data were gathered from a clinical sample receiving current psychological treatment, and as such are representative of populations encountered by mental health services within the UK. By examining domains of social connectedness separately, a more detailed understanding of the roles of identification with different groups was obtained. The findings would suggest the use of SaT or other similar Phase 1 interventions, which promote development of adaptive emotion regulation skills, are an important feature of treatment to reduce the heightened risk of suicide associated with experience of interpersonal trauma. They would also suggest that supporting individuals to develop and maintain social relationships is an important component of treatment, particularly in relation to reducing suicidal thinking. Group-based interventions such as SaT are well-suited to this by facilitating the development of adaptive coping skills and the opportunity to build relationships with others with similar life experiences. Overall, the study highlights important clinical implications in mitigating suicide risk that can be employed alongside, or in place of, psychological intervention.

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6. Appendices

Appendix 1. Journal of Affective Disorders Author Guidelines (Relevant Sections), **retrieved from** https://www.elsevier.com/journals/journal-of-affective-disorders/01650327/guide-for-authors (accessed 14.04.2019)

Preparation of Manuscripts

Articles should be in English. The title page should appear as a separate sheet bearing title (without article type), author names and affiliations, and a footnote with the corresponding author's full contact information, including address, telephone and fax numbers, and e-mail address (failure to include an e-mail address can delay processing of the manuscript).

Papers should be divided into sections headed by a caption (e.g., Introduction, Methods, Results, Discussion). A structured abstract of no more than 250 words should appear on a separate page with the following headings and order: Background, Methods, Results, Limitations, Conclusions (which should contain a statement about the clinical relevance of the research). A list of three to six key words should appear under the abstract. **Authors should note that the 'limitations' section both in the discussion of the paper AND IN A STRUCTURED ABSTRACT are essential. Failure to include it may delay in processing the paper, decision making and final publication.**

Figures and Photographs

Figures and Photographs of good quality should be submitted online as a separate file. Please use a lettering that remains clearly readable even after reduction to about 66%. For every figure or photograph, a legend should be provided. All authors wishing to use illustrations already published must first obtain the permission of the author and publisher and/or copyright holders and give precise reference to the original work. This permission must include the right to publish in electronic media.

Tables

Tables should be numbered consecutively with Arabic numerals and must be cited in the text in sequence. Each table, with an appropriate brief legend, comprehensible without reference to the text, should be typed on a separate page and uploaded online. Tables should be kept as simple as possible and wherever possible a graphical representation used instead. Table titles should be complete but brief. Information other than that defining the data should be presented as footnotes.

Please refer to the generic Elsevier artwork instructions: http://authors.elsevier.com/artwork/jad.

Preparation of supplementary data

Elsevier accepts electronic supplementary material to support and enhance your scientific research. Supplementary files offer the author additional possibilities to

publish supporting applications, movies, animation sequences, high-resolution images, background datasets, sound clips and more.

Supplementary files supplied will be published online alongside the electronic version of your article in Elsevier web products, including ScienceDirect: http://www.sciencedirect.com. In order to ensure that your submitted material is directly usable, please ensure that data is provided in one of our recommended file formats. Authors should submit the material in electronic format together with the article and supply a concise and descriptive caption for each file. For more detailed instructions please visit our Author Gateway at: https://www.elsevier.com/authors.

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The Journal of Affective Disorders is now also included in a new initiative from Elsevier: 'Colourful e-Products'. Through this initiative, figures that appear in black & white in print can appear in colour, online, in ScienceDirect at http://www.sciencedirect.com.

There is no extra charge for authors who participate.

For colour reproduction in print, you will receive information regarding the costs from Elsevier after receipt of your accepted article. Please indicate your preference for colour in print or on the Web only. Because of technical complications which can arise by converting colour figures to "grey scale" (for the printed version should you not opt for colour in print) please submit in addition usable black and white versions of all the colour illustrations. For further information on the preparation of electronic artwork, please see http://authors.elsevier.com/artwork/jad.

Peer review

This journal operates a single blind review process. All contributions will be initially assessed by the editor for suitability for the journal. Papers deemed suitable are then typically sent to a minimum of two independent expert reviewers to assess the scientific quality of the paper. The Editor is responsible for the final decision regarding acceptance or rejection of articles. The Editor's decision is final. More information on types of peer review.

Use of word processing software

It is important that the file be saved in the native format of the word processor used. The text should be in single-column format. Keep the layout of the text as simple as possible. Most formatting codes will be removed and replaced on processing the article. In particular, do not use the word processor's options to justify text or to hyphenate words. However, do use bold face, italics, subscripts, superscripts etc. When preparing tables, if you are using a table grid, use only one grid for each individual table and not a grid for each row. If no grid is used, use tabs, not spaces, to

align columns. The electronic text should be prepared in a way very similar to that of conventional manuscripts (see also the Guide to Publishing with Elsevier). Note that source files of figures, tables and text graphics will be required whether or not you embed your figures in the text. See also the section on Electronic artwork.
To avoid unnecessary errors you are strongly advised to use the 'spell-check' and 'grammar-check' functions of your word processor.

Highlights

Highlights are mandatory for this journal. They consist of a short collection of bullet points that convey the core findings of the article and should be submitted in a separate editable file in the online submission system. Please use 'Highlights' in the file name and include 3 to 5 bullet points (maximum 85 characters, including spaces, per bullet point). You can view example Highlights on our information site.

Abstract

A concise and factual abstract is required. The abstract should state briefly the purpose of the research, the principal results and major conclusions. An abstract is often presented separately from the article, so it must be able to stand alone. For this reason, References should be avoided, but if essential, then cite the author(s) and year(s). Also, non-standard or uncommon abbreviations should be avoided, but if essential they must be defined at their first mention in the abstract itself.

Graphical abstract
Although a graphical abstract is optional, its use is encouraged as it draws more attention to the online article. The graphical abstract should summarize the contents of the article in a concise, pictorial form designed to capture the attention of a wide readership. Graphical abstracts should be submitted as a separate file in the online submission system. Image size: Please provide an image with a minimum of 531 × 1328 pixels (h × w) or proportionally more. The image should be readable at a size of 5 × 13 cm using a regular screen resolution of 96 dpi. Preferred file types: TIFF, EPS, PDF or MS Office files. You can view Example Graphical Abstracts on our information site.
Authors can make use of Elsevier's Illustration Services to ensure the best presentation of their images and in accordance with all technical requirements.

Keywords

Immediately after the abstract, provide a maximum of 6 keywords, using American spelling and avoiding general and plural terms and multiple concepts (avoid, for example, 'and', 'of'). Be sparing with abbreviations: only abbreviations firmly established in the field may be eligible. These keywords will be used for indexing purposes.

Appendix 1. Journal of Affective Disorders Author Guidelines (cont′d)

Appendix 1. Journal of Affective Disorders Author Guidelines (cont′ d)

Abbreviations
Define abbreviations that are not standard in this field in a footnote to be placed on the first page of the article. Such abbreviations that are unavoidable in the abstract must be defined at their first mention there, as well as in the footnote. Ensure consistency of abbreviations throughout the article.

Acknowledgements
Collate acknowledgements in a separate section at the end of the article before the references and do not, therefore, include them on the title page, as a footnote to the title or otherwise. List here those individuals who provided help during the research (e.g., providing language help, writing assistance or proof reading the article, etc.).

Formatting of funding sources
List funding sources in this standard way to facilitate compliance to funder's requirements:

Funding: This work was supported by the National Institutes of Health [grant numbers xxxx, yyyy]; the Bill & Melinda Gates Foundation, Seattle, WA [grant number zzzz]; and the United States Institutes of Peace [grant number aaaa].

It is not necessary to include detailed descriptions on the program or type of grants and awards. When funding is from a block grant or other resources available to a university, college, or other research institution, submit the name of the institute or organization that provided the funding.

If no funding has been provided for the research, please include the following sentence:

This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors.

Nomenclature and units
Follow internationally accepted rules and conventions: use the international system of units (SI). If other quantities are mentioned, give their equivalent in SI. You are urged to consult IUPAC: Nomenclature of Organic Chemistry for further information.

Math formulae
Please submit math equations as editable text and not as images. Present simple formulae in line with normal text where possible and use the solidus (/) instead of a horizontal line for small fractional terms, e.g., X/Y. In principle, variables are to be presented in italics. Powers of e are often more conveniently denoted by exp. Number consecutively any equations that have to be displayed separately from the text (if referred to explicitly in the text).

Appendix 1. Journal of Affective Disorders Author Guidelines (cont′d)

Footnotes
Footnotes should be used sparingly. Number them consecutively throughout the article. Many word processors can build footnotes into the text, and this feature may be used. Otherwise, please indicate the position of footnotes in the text and list the footnotes themselves separately at the end of the article. Do not include footnotes in the Reference list.

Artwork

Electronic artwork
General points
• Make sure you use uniform lettering and sizing of your original artwork.
• Embed the used fonts if the application provides that option.
• Aim to use the following fonts in your illustrations: Arial, Courier, Times New Roman, Symbol, or use fonts that look similar.
• Number the illustrations according to their sequence in the text.
• Use a logical naming convention for your artwork files.
• Provide captions to illustrations separately.
• Size the illustrations close to the desired dimensions of the published version.
• Submit each illustration as a separate file.
A detailed guide on electronic artwork is available.
You are urged to visit this site; some excerpts from the detailed information are given here.
Formats
If your electronic artwork is created in a Microsoft Office application (Word, PowerPoint, Excel) then please supply 'as is' in the native document format.
Regardless of the application used other than Microsoft Office, when your electronic artwork is finalized, please 'Save as' or convert the images to one of the following formats (note the resolution requirements for line drawings, halftones, and line/halftone combinations given below):
EPS (or PDF): Vector drawings, embed all used fonts.
TIFF (or JPEG): Color or grayscale photographs (halftones), keep to a minimum of 300 dpi.
TIFF (or JPEG): Bitmapped (pure black & white pixels) line drawings, keep to a minimum of 1000 dpi.
TIFF (or JPEG): Combinations bitmapped line/half-tone (color or grayscale), keep to a minimum of 500 dpi.
Please do not:
• Supply files that are optimized for screen use (e.g., GIF, BMP, PICT, WPG); these typically have a low number of pixels and limited set of colors;
• Supply files that are too low in resolution;
• Submit graphics that are disproportionately large for the content.

Color artwork
Please make sure that artwork files are in an acceptable format (TIFF (or JPEG), EPS

(or PDF), or MS Office files) and with the correct resolution. If, together with your accepted article, you submit usable color figures then Elsevier will ensure, at no additional charge, that these figures will appear in color online (e.g., ScienceDirect and other sites) regardless of whether or not these illustrations are reproduced in color in the printed version. **For color reproduction in print, you will receive information regarding the costs from Elsevier after receipt of your accepted article**. Please indicate your preference for color: in print or online only. Further information on the preparation of electronic artwork.

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Tables

Please submit tables as editable text and not as images. Tables can be placed either next to the relevant text in the article, or on separate page(s) at the end. Number tables consecutively in accordance with their appearance in the text and place any table notes below the table body. Be sparing in the use of tables and ensure that the data presented in them do not duplicate results described elsewhere in the article. Please avoid using vertical rules and shading in table cells.

References

Citation in text

Please ensure that every reference cited in the text is also present in the reference list (and vice versa). Any references cited in the abstract must be given in full. Unpublished results and personal communications are not recommended in the reference list, but may be mentioned in the text. If these references are included in the reference list they should follow the standard reference style of the journal and should include a substitution of the publication date with either 'Unpublished results' or 'Personal communication'. Citation of a reference as 'in press' implies that the item has been accepted for publication.

Data references

This journal encourages you to cite underlying or relevant datasets in your manuscript by citing them in your text and including a data reference in your Reference List. Data references should include the following elements: author name(s), dataset title, data repository, version (where available), year, and global persistent identifier. Add [dataset] immediately before the reference so we can properly identify it as a data reference. The [dataset] identifier will not appear in your published article.

Appendix 1. Journal of Affective Disorders Author Guidelines (cont′d)

Reference management software

Most Elsevier journals have their reference template available in many of the most popular reference management software products. These include all products that support Citation Style Language styles, such as Mendeley. Using citation plug-ins from these products, authors only need to select the appropriate journal template when preparing their article, after which citations and bibliographies will be automatically formatted in the journal's style. If no template is yet available for this journal, please follow the format of the sample references and citations as shown in this Guide. If you use reference management software, please ensure that you remove all field codes before submitting the electronic manuscript. More information on how to remove field codes from different reference management software.

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Text: All citations in the text should refer to:
1. *Single author:* the author's name (without initials, unless there is ambiguity) and the year of publication;
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Citations may be made directly (or parenthetically). Groups of references can be listed either first alphabetically, then chronologically, or vice versa.
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Examples:

Reference to a journal publication:
Van der Geer, J., Hanraads, J.A.J., Lupton, R.A., 2010. The art of writing a scientific article. J. Sci. Commun. 163, 51–59. https://doi.org/10.1016/j.Sc.2010.00372.
Reference to a journal publication with an article number:
Van der Geer, J., Hanraads, J.A.J., Lupton, R.A., 2018. The art of writing a scientific article. Heliyon. 19, e00205. https://doi.org/10.1016/j.heliyon.2018.e00205.

Appendix 1. Journal of Affective Disorders Author Guidelines (cont′d)

Reference to a book:
Strunk Jr., W., White, E.B., 2000. The Elements of Style, fourth ed. Longman, New Yprk.

Reference to a chapter in an edited book:
Mettam, G.R., Adams, L.B., 2009. How to prepare an electronic version of your article, in: Jones, B.S., Smith , R.Z. (Eds.), Introduction to the Electronic Age. E-Publishing Inc., New York, pp. 281–304.
Reference to a website:
Cancer Research UK, 1975. Cancer statistics reports for the UK. http://www.cancerresearchuk.org/aboutcancer/statistics/cancerstatsreport/ (accessed 13 March 2003).
Reference to a dataset:
[dataset] Oguro, M., Imahiro, S., Saito, S., Nakashizuka, T., 2015. Mortality data for Japanese oak wilt disease and surrounding forest compositions. Mendeley Data, v1. https://doi.org/10.17632/xwj98nb39r.1.

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Data visualization

Include interactive data visualizations in your publication and let your readers interact and engage more closely with your research. Follow the instructions here to find out about available data visualization options and how to include them with your article.

Appendix 1. Journal of Affective Disorders Author Guidelines (cont′d)

Supplementary material

Supplementary material such as applications, images and sound clips, can be published with your article to enhance it. Submitted supplementary items are published exactly as they are received (Excel or PowerPoint files will appear as such online). Please submit your material together with the article and supply a concise, descriptive caption for each supplementary file. If you wish to make changes to supplementary material during any stage of the process, please make sure to provide an updated file. Do not annotate any corrections on a previous version. Please switch off the 'Track Changes' option in Microsoft Office files as these will appear in the published version.

Appendix 2. PROSPERO Protocol, ID CRD42018107675.

PROSPERO
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NHS
National Institute for Health Research

UNIVERSITY of York
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Systematic review

1. * Review title.

Give the working title of the review, for example the one used for obtaining funding. Ideally the title should state succinctly the interventions or exposures being reviewed and the associated health or social problems. Where appropriate, the title should use the PI(E)COS structure to contain information on the Participants, Intervention (or Exposure) and Comparison groups, the Outcomes to be measured and Study designs to be included.

Group-based therapies for adults who have experienced interpersonal trauma: a systematic review

2. Original language title.

For reviews in languages other than English, this field should be used to enter the title in the language of the review. This will be displayed together with the English language title.

3. * Anticipated or actual start date.

Give the date when the systematic review commenced, or is expected to commence.

19/06/2018

4. * Anticipated completion date.

Give the date by which the review is expected to be completed.

01/05/2019

5. * Stage of review at time of this submission.

Indicate the stage of progress of the review by ticking the relevant Started and Completed boxes. Additional information may be added in the free text box provided.
Please note: Reviews that have progressed beyond the point of completing data extraction at the time of initial registration are not eligible for inclusion in PROSPERO. Should evidence of incorrect status and/or completion date being supplied at the time of submission come to light, the content of the PROSPERO record will be removed leaving only the title and named contact details and a statement that inaccuracies in the stage of the review date had been identified.
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PROSPERO
International prospective register of systematic reviews

NHS National Institute for Health Research

Review stage	Started	Completed
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Piloting of the study selection process	Yes	Yes
Formal screening of search results against eligibility criteria	Yes	Yes
Data extraction	Yes	Yes
Risk of bias (quality) assessment	Yes	Yes
Data analysis	Yes	Yes

Provide any other relevant information about the stage of the review here (e.g. Funded proposal, protocol not yet finalised).

Currently being prepared for submission as part of Dr Mitchell's doctoral thesis and for publication.

Currently being prepared for submission as part of Dr Mitchell's doctoral thesis and for publication.

6. * Named contact.

The named contact acts as the guarantor for the accuracy of the information presented in the register record.

Dr Katy Mitchell

Email salutation (e.g. "Dr Smith" or "Joanne") for correspondence:

Dr Mitchell

7. * Named contact email.

Give the electronic mail address of the named contact.

katy.mitchell@nhs.net

8. Named contact address

Give the full postal address for the named contact.

NHS Tayside Psychological Therapies Service, Dudhope House, 15 Dudhope Terrace, Dundee, DD3 6HH

9. Named contact phone number.

Give the telephone number for the named contact, including international dialling code.

10. * Organisational affiliation of the review.

Full title of the organisational affiliations for this review and website address if available. This field may be completed as 'None' if the review is not affiliated to any organisation.

University of Edinburgh

Organisation web address:

www.ed.ac.uk

Appendix 2. PROSPERO Protocol, ID CRD42018107675 (cont'd)

11. * Review team members and their organisational affiliations.

Give the title, first name, last name and the organisational affiliations of each member of the review team. Affiliation refers to groups or organisations to which review team members belong.

Dr Katy Mitchell. University of Edinburgh/NHS Tayside
Dr Angus MacBeth. University of Edinburgh

12. * Funding sources/sponsors.

Give details of the individuals, organizations, groups or other legal entities who take responsibility for initiating, managing, sponsoring and/or financing the review. Include any unique identification numbers assigned to the review by the individuals or bodies listed.

The University of Edinburgh takes responsibility for supervising the review as this is completed in part-fulfilment of Dr Mitchell's Doctorate in Clinical Psychology.

13. * Conflicts of interest.

List any conditions that could lead to actual or perceived undue influence on judgements concerning the main topic investigated in the review.

None

14. Collaborators.

Give the name and affiliation of any individuals or organisations who are working on the review but who are not listed as review team members.

Dr Kate Duncan. NHS Tayside
Dr Lindsay-Jo Sevier-Guy. NHS Fife

15. * Review question.

State the question(s) to be addressed by the review, clearly and precisely. Review questions may be specific or broad. It may be appropriate to break very broad questions down into a series of related more specific questions. Questions may be framed or refined using PI(E)COS where relevant.

What group-based therapies are available for the treatment of interpersonal trauma? How effective are these therapies?

16. * Searches.

Give details of the sources to be searched, search dates (from and to), and any restrictions (e.g. language or publication period). The full search strategy is not required, but may be supplied as a link or attachment.

Research databases will be searched including: PsycINFO, Psychology and Behavioural Sciences collection, CENTRAL, CINAHL Plus, and MEDLINE. Reference lists and footnotes of relevant publications will also be search for further sources, and research authors may be contacted to identify any unpublished data.

A Google Scholar search will be conducted with the first ten pages reviewed for additional articles.

17. URL to search strategy.

Give a link to a published pdf/word document detailing either the search strategy or an example of a search strategy for a specific database if available (including the keywords that will be used in the search strategies), or upload your search strategy.Do NOT provide links to your search results.

Appendix 2. PROSPERO Protocol, ID CRD42018107675 (cont'd)

Alternatively, upload your search strategy to CRD in pdf format. Please note that by doing so you are consenting to the file being made publicly accessible.

Yes I give permission for this file to be made publicly available

18. * Condition or domain being studied.

Give a short description of the disease, condition or healthcare domain being studied. This could include health and wellbeing outcomes.

Psychological distress in relation to experience of interpersonal trauma.

19. * Participants/population.

Give summary criteria for the participants or populations being studied by the review. The preferred format includes details of both inclusion and exclusion criteria.

Inclusion:

Adults (18+ years) who experienced interpersonal trauma in childhood or adulthood. This does NOT include vicarious trauma.

Exclusion:

Those who have experienced trauma that is not interpersonal in nature.

20. * Intervention(s), exposure(s).

Give full and clear descriptions or definitions of the nature of the interventions or the exposures to be reviewed.

Inclusion:

A group-based treatment designed to treat adult participants following experience of interpersonal trauma.

Treatment goals may be reduction of symptoms/psychological distress or improvement of adaptive behaviours, e.g. emotion regulation skills.

Exclusion:

Treatments that are not group-based.

Treatments that do not have basis in psychological model.

Treatments being delivered for other types of trauma or other mental health problems.

Treatments that are provided in inpatient/forensic unit settings.

21. * Comparator(s)/control.

Where relevant, give details of the alternatives against which the main subject/topic of the review will be compared (e.g. another intervention or a non-exposed control group). The preferred format includes details of both inclusion and exclusion criteria.

None required, but relevant control may be 1:1 therapy or waiting list.

22. * Types of study to be included.

Give details of the types of study (study designs) eligible for inclusion in the review. If there are no restrictions on the types of study design eligible for inclusion, or certain study types are excluded, this should be stated. The preferred format includes details of both inclusion and exclusion criteria.

Inclusion:

Primary papers

Exclusion:

Books, book chapters, or book reviews

Personal/reflective accounts

No description of how therapy was delivered

No quantitative measure of efficacy (minimum pre-post)

23. Context.

Give summary details of the setting and other relevant characteristics which help define the inclusion or exclusion criteria.

To be included papers must provide a thorough description of the group-based treatment delivered, and some quantitative measure of its effectiveness.

24. * Main outcome(s).

Give the pre-specified main (most important) outcomes of the review, including details of how the outcome is defined and measured and when these measurement are made, if these are part of the review inclusion criteria.

Assessment of group-based treatments available for those who have experienced interpersonal trauma and the effectiveness of these treatments, either in improving adaptive behaviour or reducing symptoms/distress.

Timing and effect measures

25. * Additional outcome(s).

List the pre-specified additional outcomes of the review, with a similar level of detail to that required for main outcomes. Where there are no additional outcomes please state 'None' or 'Not applicable' as appropriate to the review

None

Appendix 2. PROSPERO Protocol, ID CRD42018107675 (cont'd)

Timing and effect measures

26. * Data extraction (selection and coding).

Give the procedure for selecting studies for the review and extracting data, including the number of researchers involved and how discrepancies will be resolved. List the data to be extracted.

Upon completion of data gathering, identified studies will be judged against inclusion/exclusion criteria by title and abstract. Remaining papers will then be judged following reading of the full text.

A standardised form will be used to extract data from the included studies. Data extracted will include: study setting; participant demographics and baseline characteristics; details of intervention and any control conditions; outcomes and times of measurement.

27. * Risk of bias (quality) assessment.

State whether and how risk of bias will be assessed (including the number of researchers involved and how discrepancies will be resolved), how the quality of individual studies will be assessed, and whether and how this will influence the planned synthesis.

Following initial reading of the included papers, a quality assessment tool will be developed with the researcher and their supervisor in order to review the quality of relevant elements within each paper. The primary author will review all papers. A random selection of the sample (25% of all included papers) will be reviewed by a second researcher independent to the review team. Agreement between the reviewers will be assessed with any discrepancies being resolved between them. If a discrepancy cannot be resolved by the reviewers, the relevant paper will be referred to the supervisor.

28. * Strategy for data synthesis.

Give the planned general approach to synthesis, e.g. whether aggregate or individual participant data will be used and whether a quantitative or narrative (descriptive) synthesis is planned. It is acceptable to state that a quantitative synthesis will be used if the included studies are sufficiently homogenous.

A narrative synthesis is planned to identify group-based treatments designed for those who have experienced interpersonal trauma and to examine the efficacy of these treatments. Should there be sufficient scope for a quantitative analysis this will be structured around the varying efficacy across treatment modalities.

29. * Analysis of subgroups or subsets.

Give details of any plans for the separate presentation, exploration or analysis of different types of participants (e.g. by age, disease status, ethnicity, socioeconomic status, presence or absence or co-morbidities); different types of intervention (e.g. drug dose, presence or absence of particular components of intervention); different settings (e.g. country, acute or primary care sector, professional or family care); or different types of study (e.g. randomised or non-randomised).

None

30. * Type and method of review.

Select the type of review and the review method from the lists below. Select the health area(s) of interest for your review.

PROSPERO
International prospective register of systematic reviews

NHS National Institute for Health Research

Type of review

Cost effectiveness
No

Diagnostic
No

Epidemiologic
No

Individual patient data (IPD) meta-analysis
No

Intervention
No

Meta-analysis
No

Methodology
No

Narrative synthesis
Yes

Network meta-analysis
No

Pre-clinical
No

Prevention
No

Prognostic
No

Prospective meta-analysis (PMA)
No

Review of reviews
No

Service delivery
No

Synthesis of qualitative studies
No

Systematic review
Yes

Other
No

Health area of the review

Alcohol/substance misuse/abuse
No

Blood and immune system
No

Cancer
No

Cardiovascular
No

Care of the elderly
No

Child health

Appendix 2. PROSPERO Protocol, ID CRD42018107675 (cont'd)

No

Complementary therapies
No

Crime and justice
No

Dental
No

Digestive system
No

Ear, nose and throat
No

Education
No

Endocrine and metabolic disorders
No

Eye disorders
No

General interest
No

Genetics
No

Health inequalities/health equity
No

Infections and infestations
No

International development
No

Mental health and behavioural conditions
Yes

Musculoskeletal
No

Neurological
No

Nursing
No

Obstetrics and gynaecology
No

Oral health
No

Palliative care
No

Perioperative care
No

Physiotherapy
No

Pregnancy and childbirth
No

Public health (including social determinants of health)
No

Rehabilitation
No

Respiratory disorders
No

Service delivery
No

Skin disorders
No

Social care
No

Surgery
No

Tropical Medicine
No

Urological
No

Wounds, injuries and accidents
No

Violence and abuse
Yes

31. Language.

Select each language individually to add it to the list below, use the bin icon to remove any added in error.
English

There is not an English language summary

32. Country.

Select the country in which the review is being carried out from the drop down list. For multi-national collaborations select all the countries involved.

Scotland

33. Other registration details.

Give the name of any organisation where the systematic review title or protocol is registered (such as with The Campbell Collaboration, or The Joanna Briggs Institute) together with any unique identification number assigned. (N.B. Registration details for Cochrane protocols will be automatically entered). If extracted data will be stored and made available through a repository such as the Systematic Review Data Repository (SRDR), details and a link should be included here. If none, leave blank.

34. Reference and/or URL for published protocol.

Give the citation and link for the published protocol, if there is one

Give the link to the published protocol.

Alternatively, upload your published protocol to CRD in pdf format. Please note that by doing so you are consenting to the file being made publicly accessible.

Yes I give permission for this file to be made publicly available

Please note that the information required in the PROSPERO registration form must be completed in full even if access to a protocol is given.

35. Dissemination plans.

Give brief details of plans for communicating essential messages from the review to the appropriate

Appendix 2. PROSPERO Protocol, ID CRD42018107675 (cont'd)

audiences.

This review will be submitted in part-fulfilment of the primary author's Doctorate in Clinical Psychology and as such will be available within her thesis. As such, results may also be disseminated at conferences and/or presentations within academic and NHS settings. Should findings be of suitable merit, the author will seek to publish these in a relevant journal.

Do you intend to publish the review on completion?

Yes

36. Keywords.

Give words or phrases that best describe the review. Separate keywords with a semicolon or new line. Keywords will help users find the review in the Register (the words do not appear in the public record but are included in searches). Be as specific and precise as possible. Avoid acronyms and abbreviations unless these are in wide use.

trauma; interpersonal trauma; complex trauma; PTSD; post-traumatic stress disorder; mental health; adult mental health; group therapy

37. Details of any existing review of the same topic by the same authors.

Give details of earlier versions of the systematic review if an update of an existing review is being registered, including full bibliographic reference if possible.

38. * Current review status.

Review status should be updated when the review is completed and when it is published. For newregistrations the review must be Ongoing.
Please provide anticipated publication date

Review_Completed_not_published

39. Any additional information.

Provide any other information the review team feel is relevant to the registration of the review.

40. Details of final report/publication(s).

This field should be left empty until details of the completed review are available.

Give the link to the published review.

Appendix 3. Adapted Agency for Healthcare Research Quality (AHRQ) Tool for Assessing Study Quality

Study Quality Assessment Tool

General Instructions: Grade each criteria as 'Yes', 'No', 'Partially', or 'Can't tell'. Factors to consider when making an assessment are listed under each criterion. Where appropriate, (particularly when assigning a rating other than 'Yes') please provide a brief rationale for your decision.

1. Unbiased selection of cohort?

Factors to consider:

- Inclusion/exclusion criteria clearly defined
- Recruitment strategy clearly described and relatively free from bias (e.g. selection bias might be introduced, for example, by recruitment via newspaper advertisement only)

Rating Guidelines:

- **YES:** clear description of inclusion/exclusion criteria that would allow replicability AND recruitment clearly described AND the strategy is relatively free from bias
- **PARTIAL:** there is some description of inclusion/exclusion criteria, but further information would be required to replicate OR there is not enough information regarding recruitment strategy to determine risk of bias OR there is some bias evident in recruitment strategy
- **NO:** no definition of inclusion/exclusion criteria OR clear bias in recruitment strategy
- **UNCLEAR:** Insufficient information provided to make an accurate rating

Note:

- Single-sex studies would not be considered as bias in this instance as interventions for interpersonal trauma are often single-sex to minimise risk of re-traumatisation/disengagement
- If a treatment is offered to all eligible patients/ppts who can then opt-in to treatment, this would be considered unbiased
- Referrals within existing health services may be unavoidable in this type of study and would not necessarily be considered a cause of bias; advertising for ppts is more likely to cause bias due to the requirement for literacy and ability to opt-in, in addition to the potential bias of advertising across some media and not others

2. Appropriateness of Comparison Group?

Factors to consider:

- Was the comparison group appropriate? i.e. Matched with the clinical group on key demographic variables? (age, gender, ethnicity, years of education)?
- Was the comparison group an appropriate intervention for treatment?
- If listed as TAU, do the authors define what this is? If so, is it an appropriate treatment for PTSD? (e.g. EMDR, CPT)

Rating Guidelines:

- **YES:** there is an appropriate comparison group AND they are either a section of the initial *N* or matched for demographics AND they received an appropriate alternative treatment AND it was clear what this treatment was
- **PARTIAL:** there is an appropriate comparison group BUT they are only partially matched for demographics OR they did not receive an alternative treatment (e.g. W/L control) OR alternative treatment not clearly described OR alternative treatment not recommended for PTSD (e.g. treatment for substance/alcohol misuse)
- **NO:** the comparison group is not appropriate (e.g. non-clinical population, not matched to sample)
- **UNCLEAR:** Insufficient information provided to make an accurate rating
- **NOT APPLICABLE:** There is no comparison group

3. Sample size calculations?

Factors to consider:

- Did the authors report conducting an a-priori power analysis or describe some other basis for determining the adequacy of study group sizes for the primary outcome of interest?
- Where a power calculation is presented, does the final sample size match this (for example within 10% of required number?

Rating Guidelines:

- **YES:** A-prior power calculation/other basis for determining sample size reported AND final sample matches this within 10% of required number.
- **PARTIAL:** A-priori power calculation or other basis for determining sample size reported but final sample size does not meet this target
- **NO:** No a-priori power calculation or other basis for determining sample size reported

4. Adequate description of the cohort?

Factors to consider:

Is the cohort well-characterised in terms of baseline (e.g.):

- Age
- Gender
- Education/Occupation/socioeconomic status
- Ethnicity
- Diagnosis/clinical/trauma status
- Marital status

Rating Guidelines:

- **YES:** reported means/SD or N/% for all or 5 excluding ethnicity
- **PARTIAL:** Reported means/SD or N/% for 3 to 4, or 5 if this includes ethnicity at the exclusion of another criteria
- **NO:** Reported 2 of the above or less

5. Validated method for ascertaining presence of PTSD symptoms?

Factors to consider:

- Was the method used to ascertain trauma symptomatology clearly described (details should be sufficient to permit replication in new studies)
- Was a valid and reliable measure used to ascertain PTSD symptomatology? Are validity and reliability of measure detailed?
- Chart diagnosis from medical notes or diagnosis made by clinical judgement in the absence of any additional measure is likely to introduce bias due to variation in how assessment is undertaken.

Ratings Guidelines:

- **YES:** method used to ascertain PTSD symptomatology was clearly described AND used a valid/reliable measure (e.g. CTQ, CAPS, PCL-C) AND the validity/reliability of the measure was detailed
- **PARTIAL:** method was clearly described BUT validity/reliability were not detailed OR further information would be required to replicate though there is some description of method and measure validity/reliability
- **NO:** Chart Diagnosis/ Review of medical notes for possible PTSD symptoms OR no description of validity/reliability of measure used OR measure used is inappropriate

Note:

- Even if authors use a measure known to be validated, such as examples listed above, they cannot be scored YES without acknowledging validity/reliability
- The measure is NOT required to make a diagnosis, only to assess presence of PTSD symptoms

6. Adequate description of group intervention?

Factors to Consider:

- Did the authors detail practical issues of group intervention, i.e. number and duration of sessions, number of participants per group, number of facilitators per group, where groups were held, format of sessions (e.g. didactic)?
- Is the type of intervention described (e.g. psychoeducational, Phase 1/2/3)?
- Do the authors provide sufficient information to determine the content of sessions?
- Is the intervention appropriate given the rationale presented?

Ratings Guidelines:

- **YES:** Practical issues are well described as such that replication would be possible (at a minimum this must include number and duration of sessions, ppts per group, facilitators per group, and format of sessions) AND the type of intervention is described AND there is sufficient information to determine the content of sessions AND the intervention is appropriate given the rationale presented
- **PARTIAL:** The authors describe only 3-4 of identified practical issues AND the type of intervention is described AND there is some information to determine the content of sessions though lacking in detail (see notes below) AND the intervention is appropriate given the rationale presented

- **NO:** The authors do not provide adequate description of practical issues (< 3 of identified issues) OR type of intervention is not described OR there is insufficient detail to determine content of sessions OR the intervention is not appropriate given the rationale presented

Note:

- If the authors refer to previous study publication for more information regarding methodology, and the description therein is adequate, they may be awarded a 'Yes' rating
- With regard to session content, information such as 'education regarding effects and symptoms of trauma'. 'teaching adaptive coping strategies such as relaxation, grounding' are sufficient. Information such as 'psychoeducation around trauma', 'coping strategies' are not.
- There is a high standard of information inclusion for this criterion; authors must meet all required information for Yes/Partial ratings, but need only meet one 'No' rating to be scored No overall.

7. Do analyses appropriately assess efficacy of group intervention?

Factors to consider:

- Was the method used to ascertain efficacy clearly described (details should be sufficient to permit replication in new studies)? At a minimum, were pre-post measures used?
- Was a valid and reliable quantitative measure used to ascertain efficacy?
- Did study authors provide evidence of effect size, not just statistical significance, to determine efficacy? Or is there another appropriate examination of clinical utility?
- Is N appropriate for analyses carried out?
- If more than one presenting problem was identified in initial methodology, was efficacy for this also assessed at a minimum of pre-post? If not, is the reason why not clearly described (and does it make sense)?

Rating Guidelines:

- **YES:** The authors used a validated and reliable symptom-specific quantitative measure of PTSD symptomatology AND this was completed at a minimum of pre- and post-treatment AND authors address clinical utility, not just statistical significance with N appropriate for analyses conducted AND any additional presenting problems are also assessed OR the decision for these not to be addressed is adequately explained
- **PARTIAL:** The authors used a validated and reliable symptom-specific quantitative measure of PTSD symptomatology AND this was completed at a minimum of pre- and post-treatment AND sample N was appropriate for analyses conducted BUT authors address only clinical utility or statistical significance, not both OR the additional presenting problems were not assessed with little/no explanation as to why
- **NO:** The authors did not use a validated/reliable symptom-specific quantitative measure of PTSD symptomatology OR this was not completed at a minimum of pre- and post-treatment OR sample N was not appropriate for analyses conducted OR authors do not address clinical/statistical significance7
- **UNCLEAR:** Insufficient information provided to make an accurate rating

Note:

- Measure of clinical utility would be some measure of effect size in addition to *p* value (e.g. Cohen's *d*)

8. Do analyses control for confounding variables?

Factors to consider:

Does the analysis control for the following factors (for example):

- Age
- Gender
- Years of Education
- Ethnicity
- Ongoing trauma or significant life events during treatment
- Presence of other mental health difficulties

Rating Guidelines:

- **YES:** Analyses control for all confounding variables AND how these are controlled for is clearly described, OR in cases where confounds cannot be controlled (e.g. life events, engagement with services such as social work or criminal justice) authors fully and clearly acknowledge this and the limitations it presents on interpretation of results
- **PARTIAL:** Analyses controls for confounding variables BUT how this is done is not clearly described OR analyses controls for only some identified confounding variables BUT this is clearly described OR in cases where confounds cannot be controlled this is acknowledged but limitations on interpretation are only partially discussed
- **NO:** Confounding variables are acknowledged but not controlled for OR there is no description of how these are controlled for OR there is no evidence that confounding variables have been controlled for OR in the case of confounds that cannot be controlled this is not acknowledged or discussed with regard to impact on interpretation of results
- **UNCLEAR:** Insufficient information provided to make an accurate rating

Note:

- Presence of other mental health difficulties can be considered adequately controlled for if these are assessed using a separate measure (i.e. PTSD and a validated measure for depression/anxiety)
- Age/gender etc. may not present as confounding variables, these are provided solely as examples
- Example confounding measures may be: additional 1:1 treatment provided alongside group treatment; other mental health difficulties (where these are not assessed by an alternative measure); varying support from additional services that may differ ppt to ppt (these would be difficult to control for but these should be acknowledged)

9. Missing data low or appropriately handled?

Factors to consider:

- Are the details of any missing data clearly reported, including how missing data was handled in the analysis? If not, is there reason to suspect missing data was present (e.g. N is lower in analysis than initially reported in participants section).
- Did missing data from any group exceed 20%?

- If missing data was present and substantial, were steps taken to minimize bias (for example, sensitivity analysis or imputation

Ratings Guidelines:

- **YES:** Missing data was clearly described and appropriate method was used to deal with this for analysis (e.g. any participant missing >20% of data excluded from analysis)
- **PARTIAL:** It would appear that missing data was excluded from analysis but the methods taken to manage this were not clearly/appropriately described
- **NO:** No information given regarding missing data or how this was dealt with for analysis or strategy used to manage missing data appears inappropriate
- **UNCLEAR:** Insufficient information provided to make an accurate rating
- **NOT APPLICABLE:** There was no missing/low data

Note:

- Intention to Treat (ITT) or Last One Carried Forward (LOCF) are appropriate ways to discuss handling of low/missing data

10. Are limitations identified and adequately addressed?

Factors to consider:

- Do the authors clearly identify limitations, either in a separate section or within a separate paragraph?
- Are the limitations discussed appropriate given the context of the study?
- Do the authors detail how these limitations may have been better addressed within the study, or how these could be addressed in future research?
- Are they any additional limitations clear in the Rater's reading of the paper that are not addressed?

Rating Guidelines:

- **YES:** Authors clearly identify limitations AND these are appropriate in the context of the study AND they detail how these could have been addressed within current methodology or in future research AND there are no other clear limitations identified by the Rater
- **PARTIAL:** Authors clearly identify limitations AND there are no other clear limitations identified BUT these are somewhat generic rather OR there is some description of how these could have been addressed but in general terms or without specific reference to either the current study or directions for future research
- **NO:** Authors do not clearly identify limitations OR they provide no discussion of how this could be addressed OR these is one or more clear limitation identified by the Rater not addressed by the authors

Note:

- Additional limitations may include any of the prior criterion that have scored Partial or No but only where these are NOT addressed by the authors, e.g. if the sample size is very small, but there is no acknowledgement of this as a limitation of the study.

Appendix 4. British Journal of Clinical Psychology Author Guidelines (Relevant Sections), retrieved from

https://onlinelibrary.wiley.com/page/journal/20448260/homepage/forauthors.html

(accessed 14.04.2019)

Author Guidelines

The British Journal of Clinical Psychology publishes original contributions to scientific knowledge in clinical psychology and Registered Reports. This includes descriptive comparisons, as well as studies of the assessment, aetiology and treatment of people with a wide range of psychological problems in all age groups and settings. The level of analysis of studies ranges from biological influences on individual behaviour through to studies of psychological interventions and treatments on individuals, dyads, families and groups, to investigations of the relationships between explicitly social and psychological levels of analysis.

All papers published in The British Journal of Clinical Psychology are eligible for Panel A: Psychology, Psychiatry and Neuroscience in the Research Excellence Framework (REF).

The following types of paper are invited:

• Papers reporting original empirical investigations

• Theoretical papers, provided that these are sufficiently related to the empirical data

• Review articles which need not be exhaustive but which should give an interpretation of the state of the research in a given field and, where appropriate, identify its clinical implications

• Brief reports and comments

1. Circulation

The circulation of the Journal is worldwide. Papers are invited and encouraged from authors throughout the world.

2. Length

The word limit for papers submitted for consideration to BJCP is 5000 words and any papers that are over this word limit will be returned to the authors. The word limit does not include the abstract, reference list, figures, or tables. Appendices however are included in the word limit. The Editors retain discretion to publish papers beyond this length in cases where the clear and concise expression of the scientific content requires greater length. In such a case, the authors should contact the Editors before submission of the paper.

3. Submission and reviewing

All manuscripts must be submitted via Editorial Manager. The Journal operates a policy of anonymous (double blind) peer review. We also operate a triage process in which submissions that are out of scope or otherwise inappropriate will be rejected by the editors without external peer review to avoid unnecessary delays. Before

Appendix 4. British Journal of Clinical Psychology Author Guidelines (cont'd)

submitting, please read the terms and conditions of submission and the declaration of competing interests. You may also like to use the Submission Checklist to help you prepare your paper.

By submitting a manuscript to or reviewing for this publication, your name, email address, and affiliation, and other contact details the publication might require, will be used for the regular operations of the publication, including, when necessary, sharing with the publisher (Wiley) and partners for production and publication. The publication and the publisher recognize the importance of protecting the personal information collected from users in the operation of these services, and have practices in place to ensure that steps are taken to maintain the security, integrity, and privacy of the personal data collected and processed. You can learn more at https://authorservices.wiley.com/statements/data-protection-policy.html.

4. Manuscript requirements

• Contributions must be typed in double spacing with wide margins. All sheets must be numbered.

• Manuscripts should be preceded by a title page which includes a full list of authors and their affiliations, as well as the corresponding author's contact details. You may like to use this template. When entering the author names into Editorial Manager, the corresponding author will be asked to provide a CRediT contributor role to classify the role that each author played in creating the manuscript. Please see the Project CRediT website for a list of roles.

• The main document must be anonymous. Please do not mention the authors' names or affiliations (including in the Method section) and refer to any previous work in the third person.

• Tables should be typed in double spacing, each on a separate page with a self-explanatory title. Tables should be comprehensible without reference to the text. They should be placed at the end of the manuscript but they must be mentioned in the text.

• Figures can be included at the end of the document or attached as separate files, carefully labelled in initial capital/lower case lettering with symbols in a form consistent with text use. Unnecessary background patterns, lines and shading should be avoided. Captions should be listed on a separate sheet. The resolution of digital images must be at least 300 dpi. All figures must be mentioned in the text.

• All papers must include a structured abstract of up to 250 words under the headings: Objectives, Methods, Results, Conclusions. Articles which report original scientific research should also include a heading 'Design' before 'Methods'. The 'Methods' section for systematic reviews and theoretical papers should include, as a minimum, a description of the methods the author(s) used to access the literature they drew upon. That is, the abstract should summarize the databases that were consulted and the search terms that were used.

• All Articles must include Practitioner Points – these are 2–4 bullet points to detail the positive clinical implications of the work, with a further 2–4 bullet points outlining cautions or limitations of the study. They should be placed below the abstract, with the heading 'Practitioner Points'.

Appendix 4. British Journal of Clinical Psychology Author Guidelines (cont'd)

• For reference citations, please use APA style. Particular care should be taken to ensure that references are accurate and complete. Give all journal titles in full and provide DOI numbers where possible for journal articles.

• SI units must be used for all measurements, rounded off to practical values if appropriate, with the imperial equivalent in parentheses.

• In normal circumstances, effect size should be incorporated.

• Authors are requested to avoid the use of sexist language.

• Authors are responsible for acquiring written permission to publish lengthy quotations, illustrations, etc. for which they do not own copyright. For guidelines on editorial style, please consult the APA Publication Manual published by the American Psychological Association.

If you need more information about submitting your manuscript for publication, please email Vicki Pang, Editorial Assistant (bjc@wiley.com) or phone +44 (0) 1243 770 410.

5. Brief reports and comments

These allow publication of research studies and theoretical, critical or review comments with an essential contribution to make. They should be limited to 2000 words, including references. The abstract should not exceed 120 words and should be structured under these headings: Objective, Method, Results, Conclusions. There should be no more than one table or figure, which should only be included if it conveys information more efficiently than the text. Title, author name and address are not included in the word limit.

6. Supporting Information

BJC is happy to accept articles with supporting information supplied for online only publication. This may include appendices, supplementary figures, sound files, videoclips etc. These will be posted on Wiley Online Library with the article. The print version will have a note indicating that extra material is available online. Please indicate clearly on submission which material is for online only publication. Please note that extra online only material is published as supplied by the author in the same file format and is not copyedited or typeset. Further information about this service can be found at http://authorservices.wiley.com/bauthor/suppmat.asp

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For authors signing the copyright transfer agreement

If the OnlineOpen option is not selected the corresponding author will be presented with the copyright transfer agreement (CTA) to sign. The terms and conditions of the CTA can be previewed in the samples associated with the Copyright FAQs.

For authors choosing OnlineOpen

If the OnlineOpen option is selected the corresponding author will have a choice of the following Creative Commons License Open Access Agreements (OAA):

Appendix 4. British Journal of Clinical Psychology Author Guidelines (cont'd)

- Creative Commons Attribution Non-Commercial License OAA

- Creative Commons Attribution Non-Commercial -NoDerivs License OAA

To preview the terms and conditions of these open access agreements please visit the Copyright FAQsand you may also like to visit the Wiley Open Access Copyright and Licence page.

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8. Colour illustrations

Colour illustrations can be accepted for publication online. These would be reproduced in greyscale in the print version. If authors would like these figures to be reproduced in colour in print at their expense they should request this by completing a Colour Work Agreement form upon acceptance of the paper. A copy of the Colour Work Agreement form can be downloaded here.

9. Pre-submission English-language editing

Authors for whom English is a second language may choose to have their manuscript professionally edited before submission to improve the English. A list of independent suppliers of editing services can be found at http://authorservices.wiley.com/bauthor/english_language.asp. All services are paid for and arranged by the author, and use of one of these services does not guarantee acceptance or preference for publication.

Appendix 5. Skewness and Kurtosis Data

Table 1.

Descriptive data for each variable, including minimum and maximum scores, and skewness and kurtosis values.

	Min.	Max.	Skewness	Kurtosis
CEA	5	25	-.397	-.621
CPA	5	25	1.051	.141
CSA	5	25	.166	-1.768
CEN	5	25	-.271	-.152
CPN	5	25	.634	-.494
DERS-SF	31	83	-.175	-.337
GISF	1.00	6.75	-.417	-.768
GISC	1.00	7.00	.082	-.574
GISO	1.00	7.00	-.557	-.785
CORE-OM	0	16	.550	-.342

Note. CEA = childhood emotional abuse, CPA = childhood physical abuse, CSA = childhood sexual abuse, CEN = childhood emotional neglect, CPN = childhood physical neglect, DERS-SF = Difficulties in Emotion Regulation Scale – Short Form, GISF = Group Identification Scale; Family domain, GISC = Group Identification Scale; Community domain, GISO = Group Identification Scale; Other domain, CORE-OM = CORE-OM risk-to-self items score.

Appendix 6. Linear Regression Model

Table 1.

Linear regression model summary across three blocked-entry models.

	R	R^2	Adj. R^2	F Change	Sig. F Change
Model 1	.390	.152	.119	4.531	.002
Model 2	.415	.172	.131	2.458	.120
Model 3	.445	.198	.140	1.545	.219

Note. Model 1 = Childhood Trauma Questionnaire (CTQ) scores for Emotional, Physical and Sexual Abuse, and Physical Neglect

Model 2 = CTQ scores for Emotional, Physical and Sexual Abuse, and Physical Neglect, and total score on Difficulties in Emotion Regulation Scale

Model 3 = CTQ scores for Emotional, Physical and Sexual Abuse, and Physical Neglect, total score on Difficulties in Emotion Regulation Scale, Group Identification Scale Family and Other group domain scores

Appendix 6. Linear Regression Model (cont' d)

Table 2.

Unstandardised coefficients beta and standard error, standardised coefficients beta, and t-values in the linear regression model using blocked entry.

Variable	Model 1				Model 2				Model 3			
	B	*SE B*	*β*	*t*	*B*	*SE B*	*β*	*t*	*B*	*SE B*	*β*	*t*
CEA	.111	.093	.152	1.199	.119	.092	.162	1.290	.070	.096	.095	.727
CPA	.014	.087	.021	.167	-.010	.087	-.015	-.115	-.009	.087	-.013	-.102
CSA	.101	.047	.214	2.160*	.084	.048	.179	1.769	.062	.050	.132	1.248
CPN	.095	.087	.127	1.093	.086	.086	.116	.997	.005	.098	.007	.051
DERS					.052	.033	.154	1.568	.068	.034	.203	1.992*
GISF									-.447	.313	-.169	-1.428
GISO									-.327	.235	-.152	-1.392

Note. CEA=Childhood Emotional Abuse, CPA=Childhood Physical Abuse, CSA=Childhood Sexual Abuse, CPN=Childhood Physical Neglect (all measured by Childhood Trauma Questionnaire, Bernstein et al., 2003), DERS=Difficulties in Emotion Regulation Scale, GISF=Group Identification Scale; Family domain, GISO=Group Identification Scale; Other domain.

*$p < 0.05$

Appendix 7. University of Edinburgh Ethical Approval

School of Health in Social Science
Clinical and Health Psychology

The University of Edinburgh
Medical School
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Telephone 0131 651 3969
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Katy Mitchell
Trainee Clinical Psychologist
School of Health in Social Science
University of Edinburgh

13 February 2019

Dear Katy,

Application for Level 1 Ethical Approval

Reference: CLIN564
Project Title: The impact of childhood trauma on suicidality, and how this is mediated by emotion regulation skills and social connectedness
Academic Supervisor: Angus MacBeth

Thank you for submitting the above research project for review by the Department of Clinical and Health Psychology Ethics Research Panel. I can confirm that the submission has been independently reviewed and was approved on the 10th February 2019.

Should there be any change to the research protocol it is important that you alert us to this as this may necessitate further review.

Yours sincerely,

Kirsty Gardner
Administrative Secretary, Clinical Psychology

Appendix 8. NHS Tayside Caldicott Approval

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Dr Katy Mitchell
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Date	25 October 2017
Your Ref	
Our Ref	IGTCAL4185
Enquiries to	Mr J. Donnelly
Extension	70249
Direct Line	N/A
Email	joseph.donnelly@nhs.net

Dear Dr Mitchell

CALDICOTT APPROVAL – Reporting the Demographic Characteristics of Referrals to the Dundee Survive & Thrive (S&T) Service through Descriptive Statistics

Proposal Sponsor: Prof. Kevin Power, Director of Clinical Psychology, NHS Tayside

Data User(s): Dr Katy Mitchell, Trainee Clinical Psychologist, NHS Tayside
Dr Kate Duncan, Clinical Psychologist, NHS Tayside

Caldicott approval is given for you to extract anonymised patient referral information from the Survive & Thrive service database and to enter it into a separate SPSS database for analysis of patient demographic factors, to inform future service provision, as described in your application and supporting information.

Thank you for your co-operation in providing us with the information requested by us in this process.
Please contact me should any queries arise from the application of this approval.

Yours sincerely

Joseph Donnelly

Joseph Donnelly
Data Protection Officer

disability confident EMPLOYER